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Soviet Political Culture and "Covert Participation" in Policy Implementation

WAYNE DIFRANCEISCO
ZVI GITELMAN
The University of Michigan

Our study of political participation in the Soviet Union, based on interviews with recent emigres, leads us to conclude that Soviet political culture is neither a "subject" nor a "subject-participant" one. There are meaningful forms of participation in the system, but they take place either outside the nominally participatory institutions, or within those institutions but in nonprescribed ways. The citizen may participate covertly, utilizing unsanctioned or blatantly illegal methods in attempts to influence policy implementation, not policymaking. The findings support the concept that traditional, prerevolutionary modes of citizen-state interactions are reinforced by the pattern of Soviet socio-economic development and by a highly centralized and hierarchical administrative structure, itself a continuation of tsarist patterns. This study describes how different types of Soviet citizens try to influence policy implementation, and how they differentiate among the bureaucracies. Analysis of this activity leads us to reformulate our conception of Soviet political culture.

The uneven, inconsistent thawing of the Soviet political system over the last 30 years has drawn the attention of Western scholars to interactions between state and society and between elites and citizens. The view of the Soviet system as a command polity, where political orders were given from above to a completely subordinated population, has been modified. We now think of more interactive politics with regularized, legitimated exchanges, however uneven, of political ideas and influence between elites and non-elites. The roles of public opinion, interest groups, and citizen participation in the formulation of Soviet policies have been much discussed.[1] Implicitly or explicitly comparing the Soviet system to Western democracies, analysts of the Soviet system have tried to determine the extent of political influence wielded by ordinary citizens, specialist elites, political functionaries at different levels, and broad social groupings such as workers or ethnic groups. The focus has been on the input: in what ways do Soviet people make demands of, and provide supports for, the Soviet polity? The first instinct of Western political scientists is to look for answers in the institutions of the system and in the informal mechanisms of policymaking. It is assumed that the crucial question is, "How and by whom is policy made?", and so one turns to voting, interest group activity, representative organs, or local government as the locus of interaction between the leaders of the polity and its rank-and-file members.

This article argues that the question of policymaking is a foregone conclusion to the great majority of Soviet people, and that the more important question to them is, "How is policy *implemented*?" Most Soviet people do not think they can make or even influence policy and are not even interested in doing so. A staunch Soviet patriot who was vigorously defending the system to one of us said that "Policymaking is none of my business—that's up to the Central Committee." He insisted this was part of a quite reasonable division of labor. Even among those who are interested in influencing policy, the majority probably sees no realistic chance of doing so.

Western scholars are divided over the extent to which citizen participation in the formal institutions of policymaking has more than symbolic value. Hough has been the most consistent champion of the view that the scope of participation in the USSR is at least as great as that in many Western democracies, and its impact on policy may be not significantly less. Hough is impressed by the growth in statistical indicators of citizens' participation in the formal organs of the system. He also suggests that the increase in participation

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[1] Some of the better known works on these subjects are Connor and Gitelman (1977), Friedgut (1979), Hough (1976), Odom (1976), Oliver (1969), Schulz and Adams (1981), and Skilling and Griffiths (1971).

may be qualitative as well as quantitative. "How do we know that citizen participation in public policy discussions is not decisive in shaping major Soviet policies?" This "remains an open question . . . the fact is we really do not know," especially since our traditional image of the USSR as a "directed society" blinds us to "phenomena such as citizen input which do not correspond to the paradigm" (Hough, 1976, pp. 7, 15, 19). (One has to wonder about Hough's own judgment when he says, in the same article (p. 13) that "there are few ideas that cannot be expressed in print in one form or another" in the USSR.)

Hough is somewhat more cautious in later formulations. He speaks of "large numbers of Soviet citizens" who "are engaged in activities that *would seem* to give them *at least* the potential of influence on *some types* of decisions" as well as of "organized group activity of a kind that *can entail potential involvement* in various levels of decision-making" (Hough & Fainsod, 1979, pp. 298-299; emphasis added). But the overall impression he conveys is of increasing and very large numbers of Soviet citizens participating meaningfully in political decisionmaking.

A different picture is drawn by Friedgut, who has studied Soviet political participation intensively and from several angles. Although not denying the quantitative indicators of participation adduced by Hough, Friedgut (1979, p. 302) imputes a very different qualitative nature to participation.

> Wherever we have been able to examine empirical findings regarding the Soviet citizen's public activity, whether from conversations with emigrants, from Soviet field surveys, or from some of the more frank and penetrating discussions published by Soviet scholars, we note a distinct lack of the dimension of citizen initiative. We find chronic recurrence of formal activity devoid of content . . . . The activization of participatory institutions has not eliminated the subject element so prominent in Soviet political culture. Conformity rather than initiative still guides the Soviet citizen. Administrative *raison d'etat* is served before community self-determination and preserves its primacy through control of both the form and content of the participatory structures of the community.

Friedgut (p. 325) admits that the potential for "citizen, as opposed to subject, participation," exists, but it has not been realized for it would require "a basic change of political values in the Communist Party leadership. Such changes are as yet nowhere visible."[2]

Bialer (1980, pp. 166-167) offers a synthesis of the two positions. He distinguishes between "high politics"—the major political issues and the actions of leaders—and "low politics"—"the decisions that directly touch the citizen's daily life, the communal matters, and the conditions of the workplace." The "average Soviet citizen" is indifferent to "high politics" and feels that it's none of his business. "The average person considers politics a separate way of life, a profession for which one is trained and paid." However, "low politics" involves a "very high proportion" of the citizenry, and it is "the very substance of the Soviet system of political participation."

This is, no doubt, a useful and accurate distinction, but it is an oversimplification. On one hand, a great many of the intelligentsia who do not actively participate in high politics seem to be intensely interested, as spectators, in the game as it is played by the Communist Party elite. There are discussions about politics at social gatherings, rumors are circulated about the private and public lives of the *verkhushka* (the men at the top), and there is substantial political gossip. On the other hand, probably only a minority of the population spontaneously involve themselves in low politics, at least in those institutions whose ostensible function is to make low level policy.[3]

As we shall see, in addition to high and low politics there is a third dimension where policy making is not the issue but policy implementation is. Here politics become individualized and privatized. People do not seek to promote or retard policies that will affect large groups, but only to have policies applied to themselves in the most beneficial way possible. In order to do this, they enter into political relationships either with input-side institutions, such as soviets or the Party, or, more frequently, with government officials on the output side—administrators or bureaucrats. Soviet politics on this level is the interaction between the citizen as client or supplicant looking for private benefit and the representative of the system interpreting and implementing policy for this individual.

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[2] Falkenheim (1978, pp. 20-22) interviewed 37 emigres from the People's Republic of China and concluded that most saw political involvement as "time-consuming, competitive with attention to their personal interests, false, empty, and most often potentially dangerous." This did not make them passive or apathetic, for they did follow the media—"if you don't grasp politics, politics will grasp you"—and engaged in the "rituals of support."

[3] Bennett (1980) suggests that policy is not only the end result of decision-making processes; it also contributes to the process of reinforcing the dominant myths of the polity and preserving its social and economic arrangements.

If Soviet political relationships are as we shall describe them, then conventional notions of Soviet political culture need to be revised. Neither the "parochial-subject-participant" trichotomy developed by Almond and Verba (1963), nor the idea of a "subject-participatory" (Barghoorn, 1972, p. 25) political culture, where structures are designed for participation but the operative culture seems to treat the citizen as a subject, apply accurately to the Soviet case. This is not a subject political culture marginally affected by participatory institutions because there is a meaningful form of participation, but it takes place either outside the nominally participatory institutions or within those institutions but in nonprescribed ways. But because this participation is limited to affecting political outputs that concern the individual directly, it would be misleading to equate the Soviet kind of political culture with those that are conventionally thought of as largely participant. Moreover, the Russian-Soviet case (like others, especially in the Third World) demonstrates that there is no ineluctable progression from parochial to subject to participant political cultures. The Soviet system, like many others, is syncretic, adapting traditional clientelist modes to what appear to be institutions for democratic participation.

One might expect a radical revolutionary regime not to adapt but to eliminate completely traditional political modes. But as Jowitt (1974) points out, Marxist-Leninist elites are induced by their ideology to select a set of system-building institutions which, ironically, reinforce traditional values and orientations toward politics. Jowitt cites three of these structural components of Communist system building and their ramifications for political beliefs at the mass level. The first component, the "dictatorship of the proletariat," with its stress on discipline, coercion, and Party control of both public and private sectors of the society, preserves much of the essence of traditional authority relationships—a bifurcation of society into the elite and a mistrusted populace. The second element involves the Leninist-Stalinist "comanding-heights" formula for development and a phenomenon that Jowitt (p. 1175) labels "revolutionary laissez-faire." Under this rubric, the emphasis is on rapid economic progress and mobilization such that the regime focuses on a rather limited set of priority areas, leaving vast segments of the society untransformed. "In return for performance in priority sectors . . . . members of society are 'allowed' to manipulate non-priority sectors for their private benefit." This lack of development in nonpriority sectors allows, as we shall see in the Soviet case, for the perpetuation of traditional clientelistic orientations toward officials, i.e., the use of *blat* (pull) or *sviazy* (connections) or even bribery. It is this pattern of interactions with public officials and institutions that we label covert participation.

A third component of Leninist system-building regimes is the elites' production mentality. The leadership believes that political culture change will inevitably follow transformation in social and economic spheres, which in practice means that the Party tends to deemphasize cultural issues except where its primary goals of socioeconomic development are affected. Of course, the Soviet Union has long passed the system-building stage, and its intensive and extensive efforts at political socialization have narrowed the gap between political structures and political culture. (Even among our sample of emigres described below, 47% acknowledge that they read agitation-propaganda material in the USSR, at least some of the time.)

Our observations on the nature of political participation and its implications for Soviet culture are based on Soviet writings and on our interviews with recent Soviet emigres. These observations lead us to conclude that the way Soviet people relate to the political administrative system is to go through the motions of participation in the nominally democratic process of making decisions, but to put far more serious effort into trying to influence the way decisions are implemented. Thus the view of Soviet political culture as subject or subject-participant is misleading. Soviet political culture is neither a democratic nor a subject one, but an amalgam of traditional, prerevolutionary modes of citizen-state relations and a superstructure of participatory institutions that superficially resemble those of Western democracies in many respects.

Sample and Method

A group of 1,161 ex-Soviet citizens who left the USSR from 1977 through 1980 were interviewed during 1980-1981 in Israel ($n = 590$), the Federal Republic of Germany ($n = 100$) and the United States ($n = 471$). The sample was drawn as a quota, nonprobability sample, in line with hypotheses that led to a certain distribution of age, sex, education, nationality, and republic of residence.[4] On some variables, such as age and sex, the proportions in the sample approximate those in the Soviet adult population rather closely. On the other hand, nearly half the respondents have had higher education (approximately 40% of all Soviet immigrants to Israel and the United States have come with higher education), 38% had secondary education, and only 15% had grade school education or less. Seventy-seven percent, or 889 people, had been registered as Jews on their internal Soviet passports, 129 as Russians, 98 as Germans, 18 as Ukrainians, and 27 as other nationalities. The areas in which the respondents had lived most of their lives are shown in Table 1.

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[4] Such a sampling strategy might not be advisable for surveying in Western countries, but it must be borne in mind that a "randomized" method of sampling the Soviet emigre population would probably not result in a higher degree of inferential power with respect to the actual Soviet population than the quota technique used here. A more conventional sampling strategy could even

Men and women are quite evenly distributed in age and regional categories, but men dominate blue collar and women white collar occupations, despite very similar educational levels. (Among the sample, 48% of the men and 46% of the women have higher education.) Educational levels are highest among those from the RSFSR, and, from the ethnic groups, among the Russians; 69% of the former and 72% of the latter have some higher education. The lowest educational levels are found among people from Central Asia (only 18% have higher education) and from Moldavia (23%). These people were interviewed in Russian or Georgian by native speakers. There were remarkably few refusals to be interviewed, although the average interview lasted between two and three hours. In addition to the standard questionnaire administered to the entire group, nearly 60 in-depth interviews were conducted with people who had been employees of the Soviet government agencies we inquired about, or who seemed to have unusually extensive knowledge about how citizens and government agencies operated in their respective republics.

The Problem of Bias

It cannot be claimed that the results obtained from any emigre sample are generalizable to the population in the Soviet Union, because not only are the emigres demographically different from the population as a whole, but presumably their attitudes and assessments are different as well. Having chosen to leave the USSR, it is reasonable to assume that they were less pleased with the system than those who stayed behind. While accepting the inadmissibility of generalizing easily from the emigre to the original population, the assumption of emigre bias can be exaggerated. First, the emigration is demographically unrepresentative not because only certain groups—Jews, Armenians, Germans—have chosen to leave the country while others have chosen to remain, but because Soviet policy in the last decade has made it possible for only those groups to leave. Therefore, the ethnic imbalance of the emigration is as much a product of Soviet emigration policy as it is of special feelings of alienation on the part of those who have left. Second, when questioned about their emigration, our respondents cited a wide variety of reasons, many of them having little or nothing to do with the Soviet political system. Thus, 23% gave as their primary reason for leaving the fact that they had relatives abroad or that they were following spouses, parents, or children who had decided to leave. Many of these secondary migrants left reluctantly, and some resent having been pulled along by the decisions of others. Another 23% cited their desire to live among people of their own ethnic group. (Among the Germans, 51% cited this reason.) Despite the possibility that some may have thought the interviewer was eager to hear about their dislike of the USSR, only 15% cited political reasons or hatred of the Soviet system as their primary reason for leaving. The only other reason cited as frequently (15%) was a desire to escape anti-Semitism or anti-German sentiment. Nearly one-fifth of the respondents gave such varied answers as to defy coding under common rubrics. These included numerous responses such as: "Soviet life had become boring"; "My sister in Israel fell ill, and I felt that I had to come and help her"; "I did not want to leave but my children did, so what could I do"; "I thought the Israeli climate would be good for my daughter's asthma"; "Everyone was going, so we went too"; "I was looking for something new in my life." (Although some of these reasons may seem trivial or frivolous, it should be remembered that travel abroad is nearly

Table 1. Respondent's Area of Residence in the USSR[a]

RSFSR	Ukraine	Moldavia	Baltic	Georgia	Central Asia
330	247	120	174	120	165

[a]The area of residence of 5 respondents was not clear.

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have very adverse consequences, if, for example, the resulting random sample of the Soviet emigre population contained grossly disproportionate distributions of such variables as sex, age, region, and occupation.

impossible for most Soviet people, and emigration becomes the only choice for people who in other countries would have many more options.) In sum, lacking any reliable data on the Soviet population's attitude toward the system, it cannot be assumed that the attitudes of the emigres are significantly more hostile, and in our sample, at least, we see that alienation from the political system was by no means the primary motivation for leaving it. Moreover, as in other emigre studies, our respondents demonstrated considerable support for many Soviet institutions and practices. One can also assume that whatever biases may be present in the sample as a whole, they are distributed fairly uniformly across population subgroups. Therefore, we anticipate that any differences observed across the subgroup strata are similar to those characterizing the same groups within the Soviet population.[5] As a further check on possible biases, we developed a hostility/alienation index from questions that solicited respondents' opinion on the Soviet system and how the individual perceived his or her relationship to the regime. This index, whose construction is discussed in Appendix 1, allows us to determine whether hypothesized relationships are significantly affected by hostility toward the system.

In any case, even alienated citizens have no choice but to participate in many ways. They are not able to avoid some of the ritualistic forms of participation, such as voting, attending meetings, joining trade unions and, usually, the Komsomol. Because the state controls so many basic necessities, they must resort to official agencies, if only for the satisfaction of their private needs. The crucial question for us then becomes not whether they participate, but how they do so, and in this there seems no prima facie reason for thinking that they differ substantially from those who did not leave the country.

The Comparative Model of Political Culture and Soviet Politics

Attempts to classify the attitudinal and behavioral orientations of Soviet citizens toward political (input) institutions usually emphasize the predominantly "subject" nature of Soviet political culture.[6] This refers, in Almond and Verba's model, to a political culture in which the typical citizen possesses an awareness of politics on the cognitive level but lacks the opportunity, the initiative, or both to influence the policymaking process (Almond & Verba, 1963, pp. 24-26). A participant culture, on the other hand, pertains to a political system in which citizens feel efficacious or politically competent and so believe that their actions to some extent affect policy decisions. Focusing on the political activities of Soviet citizens in formal settings, the conventional wisdom among Western scholars holds that participation Soviet-style tends to fulfill mainly support functions; i.e., it is primarily ritualistic and tutelary "involvement," (Sharlet, 1967). More important, it is claimed that Soviet citizens themselves recognize that their participation has little or no impact on the decisions of the *verkushka* (Friedgut, 1979; Gitelman, 1982). Thus, Friedgut (1979), Brown and Gray (1979), and others are inclined to accept Barghoorn's characterization of Soviet political culture as a "subject-participant" hybrid in which mass participation serves as a mere facsimile of democratic activity and is devoid of emotional content. Its purpose is not to give citizens a voice in decision making—on this they and the regime are agreed—but to socialize them to the regime's practices, promote their loyalty to it, demonstrate that loyalty to domestic and foreign audiences, and, perhaps, reassure the leadership that its political house is in order.

When we restricted our investigation of Soviet political participation and culture to formal, conventional modes of political communication and interest articulation, our findings tended to support this view. Gitelman adapted several instruments from Almond's and Verba's *The Civic Culture* to his survey of Soviet emigres which attempted to measure both the cognitive (awareness, knowledge) and affective (interest, competence) orientations of the respondents toward politics. Comparing these data with those from the Five-Nation samples, it was discovered that the former Soviets exhibited relatively high levels of awareness and knowledge of politics. Furthermore, even when we controlled for a putative distortion in the data resulting from the high proportion of college-educated men and women in the emigre sample, the results remained comparable to those from the Western countries studied by Almond and Verba (see Table 2). The ex-Soviet respondents were also reasonably well informed. Almost 62% of the sample correctly answered two out of three questions designed to test their knowledge of the Soviet system,[7]

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[5] For a thorough discussion of emigre research and controlling the effects of sample bias see Inkeles and Bauer (1961, chap. 2). On the last point, see pp. 26-27.

[6] See, for example, Friedgut (1979, p. 289); also Gray (1979, p. 260).

[7] The "knowledge" or "information" index was composed of three questions which asked the respondent to identify the Soviet Minister of Defense (Ustinov), the First Secretary of the Ukrainian Communist Party (Shcherbitsky), and the approximate population of the

Table 2. Following Politics Regularly and from Time to Time, by Education and Nation[a]

	Totals		Primary or Less		Secondary		Some College	
	%	*N*	%	*N*	%	*N*	%	*N*
USSR	76.3	1161	52.8	178	72.6	438	86.8	545
United States	80	970	67	339	84	442	96	188
United Kingdom	68	963	60	593	77	332	92	24
Federal Republic of Germany	72	995	69	790	89	124	100	26
Italy	36	995	24	692	58	245	87	54
Mexico	55	1007	51	877	76	103	92	24

[a]Includes data reprinted from Almond and Verba (1963, Table 10, p. 94).

Text of question: How frequently did you follow political and social events in the Soviet Union? Did you follow them regularly, from time to time, rarely or never?

whereas only 16.1% could not correctly answer any of the questions that constituted the index.[8]

Our analysis of the results on several affective variables, however, demonstrated a markedly different pattern from that found on political cognitions. Although a majority (68.2%) of the respondents reported that they "regularly" or "sometimes" discussed politics with their families and friends in the Soviet Union, only 11.2% of them claimed that they felt completely free to talk about political and social issues with anyone in the USSR.[9] Even more striking was the negative relationship between this variable and education. Only 7.9% of the respondents with some college felt uninhibited about airing their political opinions openly, as opposed to 13.7% of those with secondary and 15.2% of those with only primary schooling.

This finding seems to indicate an intense interest in politics that is combined with a privatized pattern of political communication among Soviet citizens. Accordingly, a rigid distinction is preserved between one's inner beliefs and one's public actions.[10] The data also suggest that the participation of Soviet citizens lacks an element of interpersonal trust that is essential to cooperative public activity. The results from a series of "faith in people" indicators support this inference.[11] Even more remarkable, it seems, is the negative relationship between education and affect. Although the emigres with higher education exhibit more political interest and knowledge (and also, as will be demonstrated below, a greater inclination toward participating) than the others, they are the least emotionally attached to Soviet political institutions and are less confident about their ability to influence policy decisions. To the statement, "People like me had no say about what the Soviet government did," 62% of the respondents with some college strongly agreed; the percentages for the primary and secondary school groups were 50.6 and 56.2, respectively.

Not surprisingly, levels of formal political competence among the emigre respondents were extremely low. Two *Civic Culture* questions on efficacy with respect to local-level (*soviet*) interest articulation were administered in the survey. Only 6.9% of the entire sample felt that it was possible to do something about an "unjust local law," and when probed about the likelihood of changing that law, only a few more than half of the "competents" in the first question believed they would have had anything more than a remote chance of

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Soviet Union. This latter measure was administered as a multiple-choice question in which the correct response was 225-270 million. The proportions of respondents with primary, secondary, and higher educations who correctly answered at least two questions were 52.8%, 72.6%, and 86.8%.

[8] Comparing our data with those of Almond and Verba (1963) is problematic for two reasons: First, Almond and Verba's (pp. 95-96) index of political information is not similar to that used in the Soviet emigre survey; second, their data are not broken down according to education. We must contend, however, that the data do support the inference that Soviet citizens possess a relatively fair amount of information about their system.

[9] The text of the question posed to the emigres was, "Were there people in the USSR with whom you would not discuss political and social issues?"

[10] See Friedgut (1979, p. 314). Also, see White's (1979a, pp. 110-111) discussion of the Soviet citizen's split personality, an identity for public rituals and a face shown only to his friends and family.

[11] The questions asked (for example, Can most people be trusted?, Would most people try to cheat you if they got the chance?, Could you trust most people in the Soviet Union?) elicitied similar results in all three cases; only 15 to 18% of the emigres gave the unqualified trusting response to the questions.

Table 3. Perceived Ability to Do Something about an Unjust Law, by Education and Nation (%)

	Total[a]	Primary	Secondary	College
USSR	6.9	2.8	5.0	9.7
Italy[b]	51	45	62	76

[a]Strata *ns* are the same as those reported in Table 2.

[b]Italian data are adapted from Almond and Verba (1963, Figure 1, p. 206).

success.[12] We compared the findings on "local competence" among the emigres to those in *The Civic Culture*. Although Italians possessed the lowest subjective competence levels of the five national groups studied by Almond and Verba, they greatly outdistance the Soviet sample at every level of education, as seen in Table 3.

Since these findings are based on a nonrepresentative emigre sample, we are obliged to be cautious in the inferences we draw from them. A scientific sample of the true Soviet population might well elicit higher levels of affect and competence. On the other hand, our data corroborate much of what has been shown by the current Western literature and personal observations of visitors to the USSR about the characteristic lack of enthusiasm of Soviet citizens toward the official institutions of mass participation. According to those sources, the typical Soviet accedes in the performance of his civic obligations to a great extent out of ritualistic habit, as well as in response to a variety of pressures, both blatant and more subtle, to appear loyal to the regime.[13] Further, our attempts to control for the possible biases of the emigre respondents show no significant variation between hostile and non-hostile groups in the sample on any of the affect or competence variables.

Attitudes and Participation

The preliminary findings discussed above tended to support the subject or subject-participant classification of Soviet political culture. But an analysis of the recalled behavior of the emigres casts doubt on this inference. A portion of the sample could and did press its demands on the Soviet authorities in a variety of ways. These activities ranged from writing letters to newspaper editors to contacting Party and government officials for help on personal and social problems. Citizen-initiated modes of participation require high amounts of motivation and effort by an individual (Verba & Nie, 1972, p. 52); thus, it is not surprising that only a minority of the respondents reported having engaged in such activities. More important, as Verba and Nie stipulate, the usual purpose of citizen-initiated contacts is private; the actor tends to seek a particularized output rather than a change in legal or administrative policy.[14]

A participation index was constructed from several of these citizen-contact variables plus a

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[12] The actual text of the questions was, "Let's assume that a local soviet is considering a new law which you feel is unjust—could you do anything about it?" "If you were to try to change this law on the local level, how likely is it that you would succeed?"

[13] One of our respondents, an elderly woman from Leningrad, worked as a nonstaff instructor of a district executive committee (*raiispolkom*), and also served as secretary of the electoral commission for the *raion* soviet. She disliked the agitation work connected with elections. "It was most unpleasant, especially when people realized how absurd this show was." The commission was always headed by a Party member, and its members were told that their primary obligation was to see that every resident of the district would turn out to vote. Since the polls closed at midnight, at around 10 or 11 p.m. messengers would be dispatched to the homes of those who had not yet voted. Her old, bedridden mother was the recipient of such a call. The messenger told the slightly deaf old lady, whose Russian was not very good in any case, that she had to "fulfill her civic obligation (*dolg*)." The woman became furious: "I never borrowed any money in my life and I have no obligation (*dolg*) to any one!" But, of course, she did owe the state her vote. Voting is not the only activity of this nature. Participation in political rallies and discussions or in some of the voluntary associations which abound in Soviet life may be similarly motivated, although the latter may offer symbolic and material rewards which are the prime motivation for joining and participating. See, for example, Odom (1973). On the hollowness of membership in Polish voluntary organizations, see Sadowski (1979).

[14] The emigre data on "citizen-contact" variables confirm this. The vast majority of the respondents who contacted *soviet* deputies, control commission officials, and others sought help in solving a personal or neighborhood problem, or, more often, they were attempting to procure a government service, such as employment or housing, that had not been routinely granted by the bureaucracy.

Table 4. Correlations of Political Participation with Cognitive Orientations, Affect toward Inputs and Political Competence (in Tau-b's)

Cognitive dimension	
Following Soviet politics	.28
Political knowledge	.22
Affect dimension	
Feeling free to discuss politics	–.22
Perceived influence in government policy	.06
Trust of other Soviet citizens[a]	.02
Efficacy in local politics	
Political competence[b]	.09

[a] This question asked the respondent if "most people in the USSR could be trusted."

[b] This variable was an index of the two local-competence measures discussed previously. See Appendix 3 for details on its construction.

few activities that require some initiative, but which occur in more conventional public settings, i.e., membership in the Communist Party, membership in the Komsomol, or holding an elected office (see Appendix 2). The participation scale was then tested against a variety of cognition and affect measures discussed in the last section. (For the sake of brevity we present the correlation statistics, *Tau-b*, from the analysis in Table 4.) As expected, the association measures between participation and political cognitions were fairly robust. Also, given that education was previously found to be an important indicator of a respondent's political awareness and knowledge, it is not surprising that the relationship between education and participation was strong (*Tau-b* = .41).

The findings for the relationships between the participation and the affect variables, however, did not conform to the expectations of the "rational-activist" model of political participation postulated by Almond and Verba (Almond, 1980, pp. 30-31). The negative association between participation and "feeling free to discuss politics" indicated that the most active Soviet emigres were also the most guarded in their political communications. The other correlation statistics for participation-affect and participation-competence showed weak or non-existent relationships among the variables in general. Furthermore, the *Tau-b* correlation between participation and the hostility-alienation measure was a weak .09.

The paradoxical conclusion is that those Soviet citizens who are not sanguine about the viability of formal institutions of mass participation and who are hostile or neutral toward the system as a whole are precisely those who are most self-initiated and active. We believe that this apparent anomaly can be subsumed under a rational interest rubric, i.e., that self-interest and personal need induce the participant Soviet citizen to unsolicited political activity. He eschews the formal political institutions and focuses his attention largely on the narrow issue of "Who Gets What." His participation is direct and personalized, and it is motivated by a desire for particularized outputs.

The data presented in Table 5 further illustrate the unique aspects of Soviet political activity and hint at the widespread persistence of clientelism in the political culture. Table 5 lists the responses of the emigres to a question about "the best way to influence a Soviet government decision." Despite the inclusion of the word "group" in the question, fewer than 10% mentioned forming an interest group as a viable option. The combined frequencies for officially sanctioned methods of interest articulation—"writing letters to officials" and "exerting influence through the Party"—account for only 10.4% of the respondents. The

Table 5. Best Way to Influence a Government Decision (n = 1,161)

Connections	Letters	Group	Party	Protest Demonstration	None	NR
35.5	4.5	9.9	5.9	3.4	25.8	15.2

Text of question: If a group of Soviet people were trying to influence some government decision, which way would be most effective?

modal response is "personal or family connections (*sviazy*)," at 35.5%. It seems likely that these individuals interpreted the phrase, "government decision," in terms of its implementation or output aspects—for example, the disposition of an individual case—and not in the much broader sense of policymaking, a conversion function, for which *sviazy* would be inappropriate. These findings fit the proposition stated above, that the Soviet citizen tends to avoid or denigrate cooperative activity and formal channels of interest articulation. Where he does participate he orients himself toward a specific individual or agency in an informal, and often covert, manner.

Politics on the Output Side

Thus far we have discussed two types of participation: ritualistic participation and citizen-initiated contacts with people who hold positions in policymaking institutions. In neither case is there much expectation of input into policymaking, but in the second there is some hope that action will be taken to benefit the individual, just as in the United States a congressman is asked to render constituency service without trying to influence legislation. Soviet citizens are more oriented toward the administrative side of the system. They are "subject competent," but not in the way described by Almond and Verba, that is, trying to get legally prescribed, proper treatment. According to Almond and Verba (1963, p. 162), the competent subject obeys the law, does not help shape it, and "if he is competent, he knows the law, knows what he must do, and what is due him." In the Soviet system, however, the "competent subject" is not content to demand fair play and the universal application of the law, for he does not expect that of the system. Rather he takes matters into his own hands when he is convinced that the routine workings of the system will not automatically confer upon him the benefits he desires. He does this by approaching those who implement policy, not those who make it, and by following traditional ways of handling administrators, adapted to the modern Soviet political system. "Thus a premium is placed on informal adaptive mechanisms . . . that allow for some stability and certainty in response to what is often perceived as an arbitrary and threatening regime." These mechanisms "obstruct the development of a political culture based on overt, public, and cooperative relationships. Instead they reinforce the traditional community and regime political cultures with their stress on covert, personalized, hierarchical relationships involving complicity rather than public agreements" (Jowitt, 1974, p. 1183). It may well be that the Soviet development process has actually reinforced clientelistic cultural patterns. By raising society's overall level of education to facilitate modernization, by focusing on heavy industry to the detriment of the individual's standard of living, and by failing to develop meaningful citizen participation and emotional attachment to the policymaking process, the Communist leadership has created a large number of "socialist entrepreneurs" who are highly capable of and heavily predisposed toward working nonpriority public sectors for their own benefit. Our data suggest that Soviet citizens are open to entering into informal or even illegal interactions with officials. For example, we find that three-quarters of our respondents believe that at least half of Soviet officials "derive material benefit from citizens who approach them for help," and 60% believe that a bribe could persuade a policeman to overlook a minor traffic violation. Bribery is not a last resort or an activity limited to society's marginal elements, but seems to be accepted by a large number of people as a common way of handling difficult situations. We posed an open-ended question to our respondents: "If a government official clearly lied to you or refused to give what you had coming to you, what would be the best way of making him tell the truth or giving you what was due you?" The modal response was to "offer him a gift," with 46% of the total sample mentioning this, and only 31%—the next largest group—suggesting an appeal to the official's superior. A second open question was, "What is the main precondition for success in life in the USSR?" Two responses were coded, with 42% mentioning "connections" as their first response, and 64% mentioning it either first or second. Clearly, informal, and even illegal, means are those that immediately suggest themselves to Soviet people who have to interact with the state bureaucracy.

Working the Output Side

How Soviet citizens attempt to influence actively the implementation of policy seems to vary according to two factors: their own education and the particular agency involved. Regional differences are not as great as might be supposed. Sex and age are not important in differentiating styles of confronting and dealing with the bureaucracy. The influence of education is seen in responses to the question, "Which type of government official would you prefer—the one who treats everyone equally regardless of circumstances or the one who treats each case individually, taking account of its special characteristics?"

The preference of the most educated people for a case-by-case differentiation is striking. As an

Table 6. Bureaucratic Style Preferred, by Education (n = 1,113)

	Grade School %	Secondary %	Higher %
Equal treatment	21.9	14.8	5.9
Sometimes equal, sometimes differentiated	30.9	24.7	17.4
Differentiated	36.5	55.7	73.8
Don't know, no answer	10.7	4.8	3.0

engineer from Kharkov expressed it, "Taking each case on its own merits means that the opportunity to use *blat* (pull) or *znakomstvo* (connections) is present, and that's the only way to survive in the USSR. In the United States, on the other hand, I prefer that state employees treat everyone the same." In the country of immigration, in other words, the engineer felt disadvantaged vis-à-vis the rest of the population and no longer had confidence in his ability to swing things his way in bureaucratic encounters. But in the Soviet Union educated people may think their education gives them status greater than that conferred on the bureaucrat by his position. It also gives them *savoir faire*, which they can use to their advantage. Less-educated people have no such illusions. They defer to the status conferred on the official by his position, making no judgments about the person. The more educated look at the individual and figure they can handle him because they are better educated; the less educated look at the position and are not prepared to challenge it.

However, this does not mean that they will meekly accept whatever fate, speaking through the bureaucrat, will ordain. Many people, irrespective of their educational background, try to influence the implementation of policy and the decisions of administrators, although the more educated are more likely to take an activist posture even in rigid bureaucracies such as the armed forces.

But the tactics of the more and less educated differ. Less-educated people are more inclined to bribery, whereas more-educated ones will use personal connections to extract what they want from a bureaucracy. Obviously, the highly educated are more likely to know people in high places, how to get to them, and how to approach them. This tactical difference has probably been the pattern in Russia and elsewhere for centuries; the best that the peasant could do to gain the favor of the all-mighty official was to bring him a chicken or some moonshine, whereas the educated and the wealthy were more likely to mix socially with the official and, probably, his superiors.

It is also quite clear that different agencies evoke different kinds of behavior on the part of the clients, probably not because of differences in the structure and personnel of the agencies so much as differences in the availability and nature of the services they provide, and in the importance they have been assigned by the regime.

In this study we found three categories of administrative agencies. The first category includes bureaucracies toward which citizen initiative is either unnecessary, because the agency will most likely produce the desired output without special efforts by the client, or it is useless, because the agency will not be responsive to such efforts. The great majority of respondents who had personal experience with pension agencies (*gorsobes, raisobes*) did not find it necessary to undertake any extraordinary initiatives in order to receive their pensions (although some "improved" their pensions by various means). Asked what a person should do if he did not receive a pension to which he was entitled, more than half the respondents said that a letter to a higher authority should suffice. Another 20% recommended that the person simply wait patiently, for he would surely get the pension. There was also widespread agreement that in the armed forces it would be useless to try to change one's assignment and to get around orders.

Housing falls into a second category. Although some accept the routine workings of the official housing agencies, others try to influence those workings by illegal means, and still others choose to ignore the official agencies and opt for private, legal solutions to their housing problems.

The third category of agency includes admissions committees in higher educational institutions, hiring departments of enterprises, and *raspredelenie* commissions, whose job it is to assign higher education graduates to their first post. In these institutions, it is widely felt, the routine workings of the system were highly unlikely to produce the desired result without a special "push" by the citizen which might involve semilegal or illegal measures. Thus, two-thirds of the respondents suggested bribery or using connections (*sviazy*) to avoid an undesirable job assignment, and three-quarters suggested the same tactics for gaining admission to a university or institute of higher education.

We arrived at this categorization partially on the basis of a battery of five open-ended questions consisting of hypothetical situations that asked what a (third) person could do in response to a negative decision by each of five Soviet institutions. Paraphrased versions of these questions are listed below.

Soviet Army: How could a Soviet Army officer in 1980, stationed in a good staff position in Leningrad, avoid being transferred to a unit headed for Afghanistan?

Pensions: What should a person do if he is entitled to a pension, has not received it, and is told by officials to "be patient"?[15]

Raspredelenie (assignment commission): What should a person do if he is assigned to work in a remote area after graduating from an institution of higher education?

University Admissions Committee: What should a woman do who wants to get her mediocre son into the mathematics department of a university?

Zhilotdel (local housing authority): What should a person do to get a better apartment, if the one he has is, by legal standards, large enough?

We observed that few respondents felt it advisable to resort to manipulative tactics in dealing with the army, but a large proportion suggested covert measures in dealing with job assignments, university admissions, and housing. Moreover, approximately 80% of the respondents were in the "active" range of the three variables, suggesting that regime control of these sectors is relatively lax and more willing to tolerate flexibility in them. (As we shall see, there are legal alternatives for those who fail to obtain satisfactory housing, which explains the relatively low proportion of responses in the covert range regarding housing.)

The final portion of our data analysis illustrates the combined impact of structure and education on the preferred strategy of the respondents. The findings in Table 8 follow fairly closely from our predictions. First, the higher one's education, the greater his or her sense of competence and resulting activity vis-à-vis Soviet allocative structures. This can be discerned from a glance at the column headed "passive": in every administrative setting there is a uniform decline in passivity with increasing levels of education. However, the results clearly indicate that the more powerful and strategically located is the institution, the less is the perceived opportunity to influence that institution by any means, regardless of one's social status. This is indicated by the contrast between the frequencies for the military and those for the other bureaucratic agencies.

There is ample evidence to confirm our hypothesis that the modern Soviet system has tended to reinforce traditional clientelistic orientations toward the structures of government. Soviet citizens defer to the status or power of an official and his agency as much for pragmatic as for normative reasons. Where a bureaucratic encounter can be exploited for private gain, the "competent" Soviet citizen, particularly if he or she is highly skilled and educated, will attempt to do so. There appears to be less confidence in legal rules and procedures as a means of extracting services from the state, as demonstrated by the fact that for every institution in Table 8 except the housing authority, to which legal alternatives exist, the proportions of respondents suggesting covert tactics (bribes and connections) are higher than any

Table 7. Actions Suggested by Respondents (%) (n = 1,161)

	Bribery	Connections[a]	Legal or Semi-Legal	Other	Nothing/ Passive[b]
Soviet Army	2.9	11.1	10.2[c]	3.6	72.2
Raspredelenie	19.6	37.6	10.5[d]	11.6	20.6
University admissions committee	29.4	32.7	13.8[e]	3.5	20.6
Housing authority (*zhilotdel*)	18.4	14.1	39.5[f]	5.3	22.7

[a] Includes both *protektsiia* (patronage) and *sviazy* (connections).
[b] Those who responded that one could do nothing, or who could not think of anything to do.
[c] "Threaten to resign from the army" or "plead illness or family problems."
[d] "Marry someone with the right of residence in one's home town" or "find a medical excuse."
[e] "Appeal to the committee," "apply to a different school," or "engage a private tutor."
[f] "Enter a cooperative," "exchange apartments with someone," or "buy a private apartment."

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[15] Since the question concerning attitudes toward pension agencies was asked only of those who had actually received a Soviet pension, the data could not be incorporated into Table 7. However, the responses fell within the predicted range. For example, more than 72% of those who answered advised patience or writing a letter of appeal.

Table 8. Actions suggested by Respondents, by Education (%) (n = 1,161)

	Bribery	Connections	Legal/ Semi-Legal	Other	Nothing/ Passive
Soviet army					
Primary or less	2.2	6.7	5.1	2.2	83.7
Secondary	3.7	8.2	8.2	3.9	76.0
Some college	2.2	14.9	13.4	3.9	65.3
Raspredelenie					
Primary or less	22.5	18.5	8.4	6.2	44.4
Secondary	22.1	37.7	9.6	8.2	22.4
Some college	16.7	43.9	11.9	16.1	11.4
College admissions					
Primary or less	34.8	15.7	6.2	4.5	38.8
Secondary	32.2	33.6	11.0	3.2	20.1
Some college	25.3	37.6	18.5	3.5	15.0
Housing agency					
Primary or less	19.7	9.6	34.3	7.3	29.2
Secondary	18.5	11.6	41.6	5.9	22.4
Some college	18.0	17.6	39.6	4.0	20.7

other type of approach. It should be also pointed out that all of the legal avenues suggested in regard to housing were private (enter a cooperative or exchange apartments) rather than public solutions.

The data also confirm that, although covert forms of participation appeal to all social classes in the Soviet Union, different educational groups have different preferences for strategies of influence. Bribery is (of necessity) the chosen method of the less educated, whereas *blat* is the favored instrument of the intelligentsia. In fact, for every institution, the college-educated typically outdistanced the low-status respondents in all spheres of specified activity *except bribery.* This finding confirms our hypothesis about the higher level of skills and resources available to upper-status individuals and their more expansive repertoires of tactics for pulling the right strings in their dealings with the bureaucracy.

One must exercise caution in interpreting these data, since some of the hypothetical stories we presented to the respondents more often than not involve a character who is trying to get something to which he or she is not legally entitled in the first place. One could argue that, as such, they almost force the respondents to suggest illicit and unethical activities. On the other hand, the high rate of passive responses toward the military, in contrast to the willingness of the emigres to recommend covert and manipulative tactics with respect to the other institutions, supports the decision to utilize these variables in the analysis. Furthermore, in a total of 4,644 responses to the four questions, there was not a single instance in which a former Soviet suggested that the character in the situation attempt individually or in a group to effect a change in the policy or procedure, thus corroborating the previous evidence that Soviet citizens concede that policymaking itself is a foregone conclusion. They are inclined instead to concentrate their efforts at political influence in the appropriate output sectors.

We turn now to the actual tactics used by citizens to extract their desiderata from the institutions, and observe how the nature of the institution influences the ways in which citizens will approach it. The agencies we investigated included those dealing with housing, employment, pensions, admission to higher education, the police and armed forces. Soviet sources provide ample evidence that the pension agencies are plagued by poorly trained personnel and inefficient procedures,[16] and yet we find that our

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[16] See, for example, Azarova (1979). She notes that more than two-thirds of district and city social security inspectors in the Russian republic have neither higher nor secondary specialized education. She strongly criticizes red tape, "illegal acts of employees," and the appeals process, whereby citizens are supposed to get a hearing on the size of their pensions. She goes so far as to imply quite clearly that the administration of pensions in the USSR is inferior to that in other socialist countries, citing specific examples.

Other articles along these lines are Tarasova (1976) and Tosunian (1981). The latter (p. 11) describes some of the pension officials: "Often the nature of the bureaucrat does not depend on his appearance. For some, rudeness and caddishness are the way they treat all visitors. Others are polite, well-mannered, speak softly to everyone, but they are nevertheless capable of confusing the simplest cases. Many experienced respondents evaluate the agency and its personnel favorably, and that the great majority see no need to resort to any special tactics in order to receive their pensions. The apparent paradox is explained by the fact that almost all who are entitled to pensions receive them, whereas the housing problem is perhaps the most difficult one in the daily life of the Soviet citizen. Even though the USSR has been building 2.2 million housing units annually since 1957, in the mid-1970s the average per-capita living space in urban areas was only 8 square meters (10 in Moscow). An estimated 30% of urban households still shared apartments, and it is not uncommon for people to wait as long as 10 years to get an apartment (Morton, 1980, pp. 235-236). Even getting on the list is a problem, as only those with less than nine square meters of living space (a minimum standard set in the 1920s) are eligible. Twenty percent of our respondents had been on a waiting list for an apartment.

The scramble to obtain housing is fairly general, and not a few short stories, feuilletons, and even novels have been written on the subject.[17] Small wonder that the most imaginative tactics are devised to obtain even the most modest apartments. An informant who worked in two housing administrations in Moscow in the late 1940s and 1950s, when housing was especially short, notes that bribery to obtain an apartment was so widespread that "people did not ask each other 'did you give' but only 'how much.'" Party officials, those with "responsible posts," those who had other favors to trade or simply had relatives and friends working in the housing administration were advantaged in the struggle for a dwelling. Although the situation has improved markedly in recent decades, nearly two-thirds of our respondents report that they tried to advance their position on the waiting list, either through appealing to a higher Soviet organ or, less frequently, using illegal tactics. The intervention of one's supervisor at work is often sought. Of those who went through the appeal process ($n = 129$), just over half reported that the appeal was successful and they obtained the apartment. Those who do not appeal successfully use other tactics and enter what Morton (1980) calls the "subsidiary housing market" (private rentals, cooperatives, exchanges of apartments and private houses). Exchanging apartments is the remedy most often prescribed by our respondents for those who have been unsuccessful in getting one from the official lists, but bribery is the second best. The official list is quite "flexible," as Soviet sources explain. "Too often the decisive factor is not the waiting list," *Pravda* commented, "but a sudden telephone call [after which] they give the flats to the families of football players and the whole queue is pushed back."[18] Even to purchase a cooperative apartment involves waiting lists.

A Bukharan Jewish woman from Tashkent whom we interviewed grew up in an eight-room private house with her own room. After marriage, she applied for a co-op because all her mother's children and grandchildren were registered as living in the big house, making it look like crowded conditions. The Uzbek clerk could not read Russian well and asked her to fill out the application for the co-op and then have it typed.' "When I brought the typed version I put a bottle of vodka on the desk. He didn't take money, only vodka. Uzbeks don't take money. They are very humane people. He took vodka because, as an Uzbek, he is not allowed to drink. He can't go into a store to buy vodka because the clerks are Uzbeks and it would be embarrassing. So they get vodka from us, the 'foreigners.'"

Getting a pension rarely involves this much chicanery, although the press reports numerous instances of bureaucratic snafus connected with pensions, and there are occasional reports of pension officials making money from "dead souls" in the Gogolian tradition (*Trud*, 1980). But some pensioners also monkey with the system, especially since many pensions are very low. (We have reports from Central Asia of pensions as low as 24 rubles a month, and many instances in the European USSR of pensions of approximately 60 rubles, the latter being roughly one-third the average urban wage in the 1970s.) A bookkeeper from a small town in Moldavia explained that since pensions are based on average salary in the last years of employment, "sometimes to help out a worker who was going on pension the administration would promote him to a vacancy with a higher pay scale, even if he was not qualified for the job." Bonuses and overtime pay would be

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employees are well versed in the nuances of their job, but they use their knowledge, however strange it may sound, not to benefit but to harm their clients."

[17] Examples include Plekhanov, "Order na kvartiru," *Literaturnaia gazeta,* July 25, 1979, p. 12; Ia. Ianovskii, "O sudebnoi praktike po grazhdansko-pravovym sporam mezhdu grazhdanami i zhilishchnostroitel'nymi kooperativami," *Sovetskoe gosudarstvo i pravo* No. 1, 1967; "Fiancees with Dowries," *Pravda,* January 20, 1979, translated in *Current Digest of the Soviet Press* 31, 3 (February 14, 1979); "Discussing an Urgent Problem: An Apartment for the Newlyweds," *Sovetskaia Rossia* February 14, 1979, translated in *CDSP* 31, 8 (March 21, 1979). A well-known novel on the subject is by the recently emigrated Vladimir Voinovich, *The Ivankiad* (New York: Farrar, Straus and Giroux, 1977).

[18] Cited in Morton (1980, p. 250).

calculated into the figures for average salary in order to inflate the pension. All of this, she claimed, was assumed to be legal.

Getting into an institution of higher education is a far more complicated matter, especially for Jews, in the periods from 1945 to 1958 and from 1971 to the present. Although some respondents indicate that *blat* rather than bribery is used to gain entrance to higher education, two former members of admissions committees recall the widespread use of bribery, and one woman from the Ukraine frankly said that she was admitted only because her mother paid a 3,000 ruble bribe. Another person who was on the admissions committee of a polytechnic in Leningrad reports that in his institute the bribes ran about 500 rubles, but were into the thousands for the pediatric faculty and the First Medical Institute in Leningrad. But other forms of chicanery are more prevalent. A Georgian Jew tells how he paid 100 rubles in Kulashi to have his nationality changed from Jew to Georgian so that he would be admitted to the pediatric institute in Leningrad. This trick having worked, he returned as a pediatrician to Kulashi. But when he went to change his nationality back to Jew—"everyone knew me there and it was silly to be registered as a Georgian"—"the boys" demanded 200 rubles, for, they explained, because the Jews were getting out of the country, it was now worth more to be a Jew! Our Leningrad informant, who was himself helped in getting into the school of his choice because he was a basketball player, tells us that athletes and residents of Leningrad were favored for admission, as were children of faculty. Admissions committee members in Leningrad got written instructions not to admit anyone to the journalism faculty without recommendations from the Party *raikom*. Certain specialities even in the philological faculty were explicitly closed to Jews. In such cases, bribery, connections, and other tactics will not work, except very rarely, and people learn quickly to give up on these institutions.

The other side of this is an "affirmative-action" program designed to increase the number of natives in the republic's higher educational institutions. Two Soviet authors assert that "It is understood that in socialist societies objectively there can be no discrimination against any national group. Soviet educational practice knows no such examples." At the same time, they say that, "It must be assumed that the more the proportion of a nationality in higher education corresponds to its proportion in the population as a whole, the more the system of higher education lives up to the democratic ideal of equal educational opportunity for all people irrespective of nationality." To achieve this "one can permit . . . conditional influence of a variable such as the nationality of an individual" on admissions decisions (Prikhodko & Pan, 1974, pp. 70, 61). Indeed, informants from two cities in Moldavia reported independently that in the 1970s they were told quite openly not to bother applying to Kishinev Polytechnical Institute because that was being reserved for ethnic Moldavians. Central Asian respondents portray admissions officials desperately trying to fill ethnic quotas. One woman draws a perhaps exaggerated picture of Uzbek officials scouring the countryside for young Uzbek women who could be persuaded to attend a pedagogical institute training music teachers for elementary schools. Other informants report that in the Ukraine and Moldavia, at least, rural students were favored for admission to institutes and were eagerly recruited, and this is confirmed as policy by official sources.

For those departments and schools that are realistic possibilities for Jews, the way in is not always a direct one. A common practice is to hire a tutor for the applicant, not so much to prepare the applicant as to prepare the way with the admissions committee. Often, the tutor is a member of the faculty, and he will see to it that his student gets in, sometimes by turning over some of his fees to his colleagues (reported in Moscow, Kharkov, Leningrad). One operator told parents: "I'll get your child into the institute for 1,000 rubles. Give me 300 now and the rest only if he gets in." The advance would be used to bribe clerks to put the child's name on the list of those admitted, bypassing the admissions committee, and then the rest was pocketed by the fixer. One admissions committee member admitted frankly that he gave higher admission grades to students who had been tutored by his friends.[19]

If citizens and members of admissions committees fool with the system, so, of course, does the Party. A woman who taught in several pedagogical institutes reports that at the final meeting of the admissions committee a representative of the Party *raikom* and another of the *obshchestvennost* (usually someone working with the Party) would come and express their opinions freely. They would insure that certain ethnic distributions were achieved and that certain individuals were admitted or turned down. In Kharkov, it is claimed, there are three lists of applicants: those who must be admitted, those who must not be, and the rest. In the Kharkovite's experience, the Party did not directly participate in the admissions process, but did so indirectly by approving members of admissions committees, making up the above-mentioned lists, and providing written guidelines for admission policies.

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[19] Corruption is involved in admissions even to military schools. *Krasnaia zvezda* reports a case where a general got his relatives admitted despite their poor grades and admits this is not an isolated case. "When applications to the military school are being considered the admissions committee is besieged with phone calls. . . . There are really two competitions for admission: the regular competition and the competition of relatives" (Filatov, 1980, p. 2).

The Soviet press does not hide the fact that the struggle for admission to higher education is a fierce one, and that all kinds of means are employed in it. "Every summer when the school-graduates boom starts and the doors of *vuzy* (higher educational institutions) are blocked by lines of applicants, ripples of that wave sweep over editorial staffs as well. Parents and grandparents of school graduates call up and come in person (the person who failed the exams never comes). With great inspiration they tell what profound knowledge their child has, how diligent he was, how well he replied to each question, but the perfidy of the examiner was beyond all expectations." The writer notes, however, that "the majority of complaints are quite just" (Loginova, 1980, p. 11).

The intelligentsia is especially anxious to have its children gain higher education. In Azerbaijan none other than the first secretary of the republic Party organization, now a member of the All-Union Politburo, Gaidar Aliev (1981, p. 10) complained that in the law faculty of the local university, "We discovered that the overwhelming majority of the students are children of militia, procurators, judges, law professors and employees of Party and state organs. . . . We were concerned with the threat of nepotism and 'heredity' within the administrative organs." He complained also about the fashion of the 1960s, when senior officials "arranged" to receive higher degrees, commenting sardonically on a popular saying that, "A scholar you might not be, but a *kandidat* you surely must become."

If one gets into the institute or university and then graduates, a *raspredelenie* commission will normally assign the graduate his or her first job. Very often this is an undesirable position in an even less desirable location. For example, it is common practice to assign teachers or physicians, many of whom are single women, to rural areas in Siberia and Central Asia. To avoid such assignments, some will simply take a job outside their field, others arrange fictitious marriages with spouses who have residence permits in desirable locations, and many will appeal the decision and try to get a "free diploma," that is, one without a specific job assignment, leaving them to their own devices. In only one instance were we told of a bribe being used (in the West Ukraine) to get a good assignment. Several informants report being assigned to jobs in Central Asia, only to find upon arrival that there was no need for them, that the local institutions had not requested them, and the local authorities were not eager to have non-natives take jobs there. Despite the inconvenience, such *contretemps* were welcomed because they freed the person from the assignment. In 1979 nearly 30% of assigned jobs were not taken (*Uchitel'skaia gazeta*, 1980, p. 2), and in some rural areas the proportion of those who did not show up to their assignments was higher.[20] Of course, some graduates try to use *blat*, to try and pull strings with the job assignment commission, and this is reported to work fairly well. The other use of *blat* is to get some big boss to request the graduate specifically as an employee of his institution.

The Use of Blat and Protektsiia

Since *blat* and *protektsiia* are so commonly used, they are frequently commented on by the Soviet press. One detailed analysis raised both principled and pragmatic objections to *protektsiia*. It is said to be objectionable because it violates the socialist principle of "from each according to his capabilities, to each according to his work." On the practical level, *protektsiia* is said to reward the incompetent, discourage hard work and initiative, allow people to make buying and selling favors their profession, and promote calculations of self-interest "incompatible with communist morality." The resort to *protektsiia* arises, it is suggested, because social norms are not well defined and because of the "underdevelopment of certain branches of our economy." The law is said to be too vague for curbing the use of *protektsiia*. Unlike bribery, using *protektsiia* is not generally considered a crime except if "substantial harm is done to state or public interests, or to the rights of individuals" (Kiselev, 1981, p. 152).[21]

As this argument implicitly acknowledges, the use of *protektsiia*—and in some areas and under certain circumstances, even of bribery—is socially acceptable and not discouraged by law or custom.

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[20] In Orel province in 1979 only 179 of 323 graduates of agricultural institutes showed up to their assigned jobs. some "'signed in' only to vanish immediately afterward. . . . In all fairness it must be said that not all farm managers create proper conditions under which young specialists can work. . . . In other cases they simply 'forget' to provide them with apartments . . . leave them on their own to solve all the problems of everyday life." See Troyan (1980).

[21] See the frank article by the first secretary of the Georgian writers' union, Tengiz Buachidze (1975, p. 12).

It is in line with age-old traditions in many areas of the USSR. A Georgian author shows how traditional birthdays, weddings, mourning rituals, the departure of young men to the army, and even funerals are occasions for trading influence and subtle forms of bribery (Dzhafarli, 1978, p. 72; see also Verbitskii, 1981, p. 2). Soviet authors decry "survivals of the past" which are said to contradict "socialist morality and way of life." Some Western observers see not just survivals but a Soviet failure to resocialize the population to Marxist-Leninist norms. One student of Soviet political culture asserts that "'New Soviet man,' in short, does not yet exist; Soviet citizens remain overwhelmingly the product of their historical experience rather than of Marxist-Leninist ideological training" (White, 1979b, p. 49). This is an exaggeration—there has been successful resocialization in many areas of life—but it is true that prerevolutionary styles and practices survive in certain spheres, even among third and fourth generation Soviet citizens. The relationship between the government official and the citizen closely resembles pre-Communist forms in the USSR and other socialist countries. Jowitt's (1974, p. 1176) argument that traditional attitudes and behavior patterns survive the Communist revolution—and are even reinforced by it—is borne out by our investigation.

The prevalence of *blat* should not be attributed to some mystical staying power of prerevolutionary political culture. Rather, it is supported by present-day structural factors which are themselves continuations of tsarist practices. The highly centralized and hierarchical administrative structure of tsarist days has been continued and reinforced by its heirs, so the kind of tactics used to ameliorate the harshness of tsarist administration are well suited to the present day as well. In the light of weak rational-legal authority and of interest groups in both historical periods, the average citizen is without influence over policymaking and has little legal protection against administrative arbitrariness or even the mindless application of what is construed as the law. He is left to devise individual strategies and tactics which will not change the making of the law, but will, he hopes, turn its implementation (or nonapplication) in his favor. Each person, then is reduced to being a special pleader, and not with those who make the rules but with those who are charged with applying and enforcing them.

Conclusion

Our analysis of the modes and meanings of participation in the Soviet system leads us to reject both Soviet claims for their system as well as Western characterizations of Soviet political culture as "subject" or "subject-participant." Three modes of participation in the Soviet political system have been identified: formal-ritualistic participation, which might better be labelled involvement; citizen-initiated contacts with official persons and institutions whose task is, among other things, to represent, check up on (*kontrol'*), and run interference for people; and contacts with those whose task is to implement policies made elsewhere. Our sample of emigres, and perhaps the Soviet population as well, shows little belief in the power of formal-ritualized participation to influence policy. Although a substantial number did contact official representatives and institutions, most were not greatly pleased with the outcome of these contacts, and many expressed skepticism about their utility. More confidence was expressed in the ability of the ordinary citizen to influence the process of policy implementation. Although some bureaucracies were judged quite impervious to citizen influence or manipulation, and others were found to operate so routinely that such manipulation was rarely necessary, several agencies were seen as not producing outcomes favorable to the client unless he made efforts to influence the way officials interpreted and applied policy. The citizen approached the agency either as a supplicant pleading for particular treatment, or as one who had something to exchange—favors, influence, even money—in return for favorable bureaucratic action.[22] How the citizen approaches the organization and what he offers in return for its favors depends largely on his educational background and on the particular organization involved. Those with higher education are more likely to take the initiative vis-à-vis the bureaucracy and to mobilize connections to obtain a favorable outcome. Those with less education are not as confident of their ability to sway the bureaucracy by their own efforts, but when they do so, they are more likely to resort to showing their appreciation with gifts or money bribes. The attitudes and behavior of the emigres, and probably of the Soviet citizenry, remain clientelistic and privatized, in contrast to the nominally democratic structures and collectivist norms of the official Soviet system. Our findings repeatedly suggest that the dominant social ethos of Soviet citizens vis-à-vis their government is one of private self-interest. Furthermore, they display clear preference for informal access to and influence on bureaucratic officials and a general disdain for formal and legalistic procedures and norms. This observation leads us to think of the Soviet political culture (or at least the dominant subculture) as covert-participant. The covert-participant individual is oriented toward system outputs, but he exhibits few of the deferential, passive attributes of the classic subject. Rather, he participates in (or more precisely, attempts to manipulate) the implementation process in whichever institutions he can, utilizing a varied repertoire of assertive, creative, and illegal methods to secure his private welfare from the extensive Soviet public sector. Our research also indicates that covert orientations are to be found in every stratum in the USSR, but that different educational groups prefer different manipulative strategies, and these variations are probably rooted in both tradition and in the structure of the confrontation between citizens and the state. In a critique of *The Civic Culture*, a distinguished Polish social scientist who has considerable firsthand experience with the workings of his country's political system notes that "Some social groups feel . . . that their chances of performing effectively within the system are minimal or nil; in this case political apathy may be interpreted in terms of the critical evaluation of the existing system rather than in terms of the psychological characteristics of inactive citizens" (Wiatr, 1980, pp. 116-117). He suggests that Almond and Verba err in their "tendency to explain discrepancies between normative standards of democracy and political reality in terms of psychological deficiencies rather than structural conditions within the system." Though Wiatr makes these points with regard to Western democracies, they seem equally applicable to the Soviet Union and other socialist countries. Rational political behavior in the USSR should involve pro forma participation in the system's rituals, occasional contacting of approved agencies in approved ways in order to influence policy implementation in individual cases, and more frequent transactions with officials charged with policy implementation for the same purpose. Ritualistic participation is rational, not because it influences policy, but because it protects one against charges of nonconformity and antisocial attitudes, and for some it may provide emotional satisfaction. For others, however, the effect is to emphasize the gap between rhetoric and reality and to reinforce political cynicism.[23] Despite the Khrushchevian rhetoric of the "state of the whole people" succeeding the "dictatorship of the proletariat," only the formal franchise has been broadened in the last decades. The Soviet citizen participates politically in several ways, but, except for a small elite, his (and especially her) ability to influence policy decisions, even indirectly, is practically nil. The citizen does have some ability to influence the implementation of policy. But this can be done only on an ad hoc and ad hominem basis, so that no systemic effects and changes are felt. Despite the expansion of opportunities for formal participation and the grudging increase in opportunities for expressing opinions, the Soviet system remains fundamentally directed from above. As Verba and Nie (1972, p. 113) comment in their analysis of political participation in America:

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[22] On the concept of the citizen-bureaucrat encounter as an "exchange relationship," see Hasenfeld and Steinmetz (1981). See also Hasenfeld (1978). More broadly, see Katz and Danet (1973).

> Particularized contacts can be effective for the individual contactor but they are inadequate as a guide to more general social policy. . . .The ability of the citizen to make himself heard . . . by contacting the officials . . . represents an important aspect of citizen control. Though such contacts may be important in filling the policy gaps and in adjusting policy to the individual, effective citizen control over governmental policy would be limited indeed if citizens related to their government only as isolated individuals concerned with their narrow parochial problems. The larger political questions would remain outside popular control. Therefore, though electoral mechanisms remain crude, they are the most effective for these purposes.

For the foreseeable future the "larger political questions" will remain the domain of the *verkhushka*. Our respondents appear to be much more interested in private benefits than in democratic institutions. We infer from our analysis of respondents' evaluations of Soviet bureaucracies and their dissatisfaction with its operations, that much of the Soviet population would probably be more interested in increasing levels of performance by the present system than in fundamental systemic change. Until such time as either of these comes about, the citizen is left to grapple as best he can with those small questions of daily life that he and those who administer the system must solve together.

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[23] Unger's (1981, p. 122) interviews with 46 former Soviet political activists of the Party and Komsomol lead him to conclude that "they did not believe their own participation to be effective. . . . The combination of compulsion and formalism which characterizes participation in the Komsomol and party arenas clearly provides no scope at all for the development of a sense of efficacy. Indeed, one may well hypothesize that it has the opposite effect, that the induction of the individual into the 'spectacle' of Komsomol and party activities produces not a sense of efficacy but of inefficacy, not subjective competence but subjective incompetence."

Appendix 1. Construction of the Hostility/Alienation Scale

The hostility/alienation scale comprises four measures that attempt to elicit the respondent's overall evaluation of or affect toward the regime and its institutions. These questions are listed below:

1. "Sometimes politics and government in the Soviet Union seemed so complicated that a person like me could not really understand what was going on." Do you agree strongly, agree, disagree, or disagree strongly with the statement?
2. "The Soviet press, radio, and television never tell the truth." Do you agree strongly, agree, disagree, or disagree strongly?
3. "Which of the following statements most accurately describes your relationship to the Soviet regime?" At one time I was favorably disposed, but then became opposed ("mildly hostile"). I was always opposed to the Soviet regime ("very hostile"). I was always in favor of the Soviet regime ("very favorable"). I once opposed the Soviet regime, but then became favorable to it ("mildly favorable"). I was indifferent, neutral, didn't care (coded as missing).
4. "To what extent did you consider the Soviet Union your own country?" Always, sometimes, rarely, never?

The frequency distribution for the dichotomized hostility alienation scale is: Hostile Group, 36.1% (419); Non-Hostile Group, 63.9% (742).

Appendix 2. Construction of the Political Participation Scale

The participation scale consists of variables which denote two different styles or modes of political activity. The first mode of participation is what Verba and Nie (1972) call citizen-initiated contact. The second type of variable denotes activity that is much more support-oriented than citizen contacts. The variables used in the construction of the scale are listed below:

Citizen-initiated activities
  writing a letter to the editor of a Soviet newspaper
  contacting *ispolkom/raiispolkom* official
  contacting a procurator
  contacting a People's Control Commission
  contacting an elected soviet deputy

Support-related activities
  holding an elected office
  membership in the Communist Party
  membership in the Komsomol

The scale itself is a simple summation of the number of activities of both types that the individual engaged in. Respondents who had not performed any activities were labeled Passives; those who performed either one or two acts were low participants; those who reported three or four acts were labeled Moderates, and those who engaged in more than four activities were high participants. The frequencies for the scale are: Passives, 23.9% (278); Low, 62.5% (726); Moderate, 11.5% (134); High, 2.0% (23).

Appendix 3. Construction of the Local Political Competence Scale

The political competence measure consists of the two instruments from Almond and Verba (1963) on local competence: the perceived ability to "do something about an unjust law" and the estimated likelihood of changing that law on the local level. We recoded the variables so that response levels were equivalent, and summed each respondent's scores on the two variables. Those who were positive of their efficacy in both questions were coded in the high range of the political competence scale; those whose answers were mixed were placed in the medium category; and those who responded negatively to both questions were placed in the low range: Low, 91.8% (1018), Medium, 5.3% (62); High, 2.8% (33).

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Explaining African Military Coups d'Etat, 1960-1982

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The purpose of this article is to contribute to the theoretical understanding of African military coups d'etat. We begin by replicating a well-known model (Jackman, 1978) that purports to identify the structural determinants of coups d'etat within the states of Sub-Saharan Black Africa. When the research problem is changed slightly to focus exclusively on military coups, we find major weaknesses in the original Jackman model. We then extend and refine this model and thereby account in a theoretically meaningful fashion for 91% of the variation in military coups within 35 Black African states from 1960 through 1982. Our major substantive findings indicate that Black African states with relatively dynamic economies whose societies were not very socially mobilized before independence and which have maintained or restored some degree of political participation and political pluralism have experienced fewer military coups, attempted coups, and coup plots than have states with the opposite set of characteristics.

Who is to guard the guards themselves? (Juvenal, circa 60 AD; cited in Welch & Smith, 1974, p. 1)

Clubs become trumps when no rule of trump is established. (Thomas Hobbes, cited in Zimmerman, 1979a, p. 406)

The class that bears the lance or holds the musket regularly forces its rule upon the class that handles the spade or pushes the shuttle. (Gaetano Mosca, cited in Welch & Smith, 1974, p. 8)

In this country there are, in the end, only two parties, the civilians and the soldiers. (then President of Nigeria, Shehu Shagari, 1979, cited in *Time*, January 16, 1984, p. 25)

Harold Lasswell (1965, p. 3) has described political science as "the study of changes in the shape and composition of the value patterns of society." In a famous earlier essay he wrote that to study politics one must focus on the exercise of influence and the influentials who exist within societies and who shape societal value patterns by their determination of who gets what, when and how (Lasswell, 1936).

The involvement of the military in determining who gets what, when and how in Africa south of the Sahara is all too apparent when one considers the rash of military interventions into government and politics in these very weak states (Jackson & Rosberg, 1982) since they gained independence.

Our recently collected data indicate that from 1960 through 1982, of the 45 majority-ruled states of Sub-Saharan Africa 25 (55%) have experienced 52 successful military coups d'etat. There were also 56 attempted coups and 102 reported coup plots. In sum, 38 of the 45 countries (84%) saw some form of military intervention between the beginning of 1960 and the end of 1982. The African military coup d'etat has accomplished the transfer of power and influence from one set of influentials to another much more frequently than have elections and other forms of constitutionally sanctioned regime change. *For better or worse, the study of national politics in Black Africa is rapidly being reduced to the study of military intervention and military rule.*

Coups d'etat, their causes, and their consequences have long been of intense interest to both

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policymakers and scholars.[1] An ability to explain the conditions that prompt coups and perhaps thereby to predict their occurrence would greatly contribute to our understanding of the dominant mode of governmental change in Black Africa. Explaining military coups is not an easy task, however, because they are clearly the result of a complex mix of historical, political, personal, economic, military, social, ethnic, and cultural factors.[2] There is no single, easily identified variable that can be used to explain or to predict the likelihood that African militaries will seize state power (Zimmerman, 1979a, b), because these states manifest divergent historical experiences, cultural patterns, and political economies, and this diversity is increasing each year.

Given the present state of our knowledge and theories, it is not surprising that point predictions that a coup will occur in a given country within a given time are infrequent and dubious. Paradoxically, our admitted inability to predict the occurrence of specific coups has led some scholars also to argue that coups cannot be explained systematically. For example, Zolberg (1968a) has argued forcefully that coups in Africa are a random phenomenon unrelated to structural characteristics of African societies and their international settings. Coups may occur anywhere at anytime, and according to Zolberg the best that scholarship can do is to reconstruct what happened and why on a case-by-case basis after the fact. Similarly, Decalo (1976) sees coups to be related only to one common factor, the idiosyncratic personal rivalries and interests of the coup makers and their opponents. Without knowledge of these personal networks of friendship and enmity, coups cannot be explained, much less predicted, for like Zolberg, Decalo sees them to be unrelated to structural phenomena.

The research reported in this article adopted an alternative approach. We focused on the identification of the internal and external structural conditions and trends that in combination will permit us to explain theoretically military interventions in the states of Sub-Saharan Africa during the period 1960-1982. We address the problem of prediction beyond the end of 1982 elsewhere (Slater, Johnson, & McGowan, unpublished).

Replication

We are not alone in believing that coups d'etat are systematically related to societal characteristics and the international settings of societies.[3] Our review of the literature led us to conclude that the most successful published systematic study of the African coup d'etat was Robert W. Jackman's (1978) article, "The Predictability of Coups d'Etat: A Model with African Data." Our first step, then, was to attempt to replicate Jackman's results with our new, expanded data set.

In order to measure as reliably and validly as possible the incidence of coups d'etat for 30 Black African states from 1960 through 1975, Jackman (1978, p. 1264-1265) followed the earlier research of Morrison et al. (1972, p. 128) and gave each state the sum of one point for each reported coup plot, three points for each attempted coup, and five points for each successful coup (see Table 2 for his cases and his resulting index scores). His final index ranged from zero for The Gambia to 46 for Benin (ex-Dahomey). The mean value of his index was 13.97, with a standard deviation of 12.29, and it was therefore slightly positively skewed.

Jackman used six different news chronologies to collect his data and elsewhere discusses their usefulness as sources of information on African coups and associated elite instability events (Jackman & Boyd, 1979). The reliability of his index, which includes "all" reported coup-related events whether or not military elements were involved, was demonstrated by correlating it with Morrison et al.'s (1972, p. 128) earlier independent index of elite instability. The resulting *r* was .95, an excellent level of reliability. This index, then, became Jackman's dependent variable.

As Table 1 (Col. 1) shows, Jackman's statistical model was quite robust. With only four substantive independent variables and three interaction terms he was able to explain 84% of the variation in coups d'etat across 29 Black African states from 1960 through 1975. Even more important, however, Jackman's model is strongly grounded theoretically.

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[1] General studies are Andrews and Ra'anan (1969), Finer (1967), First (1970), Gutteridge (1969), Huntington (1957, 1962, 1968), Janowitz (1977), Lloyd (1971), Luttwak (1968), Nordlinger (1977), Perlmutter (1977), and Thompson (1972, 1973).

[2] For African case studies that illustrate this, see Apter (1969), Baynham (1980), Bebler (1972), Bennett (1973), Bienen (1968), Clapham (1968), Decalo (1973, 1974 1976), Dent (1975), Feit (1969), First (1971), Fisher (1969), Hansen and Collins (1980), Higgott and Fuglestad (1975), Laitin (1976), Lewis (1972), Liebenow (1981), Lofchie (1972), Nelkin (1967), Okolo (1981), Pachter (1982), Southall (1975), Terray (1964), Tixier (1966), Twaddle (1972), Welch (1967, 1970), Wolpin (1980), and Yannopoulos and Martin (1972).

[3] O'Kane (1981), Putnam (1967), Schmitter (1971), Sigelman (1974), Thompson (1975a,b, 1976, 1980), Wayman (1975), and Zimmerman (1979a,b) have examined structural factors with respect to Latin America or for states in general; Barrows (1976), Collier (1978), Duvall and Welfling (1973), Jackman (1978), McGowan (1975), Morrison and Stevenson (1971, 1972a, b), and Wells (1974) have done the same for Africa.

Basing his argument on Deutsch (1961), Jackman (pp. 1262-1263) posited that early, rapid social mobilization of African societies should have a destabilizing effect on their post-independence governments, and his results (Table 1, Col. 1) confirm the Deutsch-Jackman hypothesis. Jackman (p. 1263) also reasoned that cultural pluralism could conceivably have three effects on the incidence of coups in tropical Africa. First, ethnically fragmented societies are inherently unstable (Rabushka & Shepsle, 1972, among others). Second, cultural pluralism may interact with other factors, such as social mobilization, to create nonadditive effects on instability. Finally, cultural pluralism may be seen as analogous to political pluralism, thereby promoting stability as long as no single ethnic group is proportionately dominant within society (Coleman, 1960, p. 368).

Jackman's results indicate that a dominant ethnic group (the opposite of cultural pluralism) increases instability in African states, and that ethnic dominance interacts with multipartyism (party dominance) to increase instability further

Table 1. Regressions of Coups d'Etat, 1960 to 1975, on Mobilization, Pluralism, Party Dominance and Turnout

Variables	Jackman's 1978 Result[a]	Our First Replication[a]	Our Second Replication[b]
Social mobilization (M)	.227 (.073)	22.729 (7.287)	25.330 (6.641)
Ethnic dominance[c] (C)	27.666 (7.142)	27.666 (7.142)	30.382 (6.425)
Party dominance (D)	−.202 (.091)	−20.218 (9.088)	−15.470 (7.316)
Turnout[d] (P)	−23.047 (7.838)	−23.047 (7.838)	−20.789 (7.379)
$D*P$	.186 (.111)	18.643 (11.161)	15.760 (10.629)
$C*D$	−.317 (.104)	−31.702 (10.437)	−34.963 (9.725)
$C*P$	11.380 (4.529)	11.380 (4.529)	11.187 (4.502)
Constant	19.129 (7.326)	19.129 (7.326)	14.732 (5.738)
N	29	29	30
R^2	.843	.843	.837
$\bar{R}^2$	.791	.800[e]	.794[e]
F	not reported	16.12	16.16
p of F	not reported	.0001	.0001

[a] Ethiopia excluded.

[b] Ethiopia included.

[c] A binary variable equals 1 when the largest ethnic group constitutes at least 44% of the population, and zero otherwise.

[d] A binary variable that equals 1 when turnout in the last election before independence was more than 20% of the population, and zero otherwise.

[e] Our formula for calculating $\bar{R}^2$ is taken from Pindyck and Rubinfeld (1981, p. 80). $\bar{R}^2 = 1 - (1 - R^2)(N - 1/N - k)$, where k is the number of independent variables. Main table entries are the parameter estimates, and numbers below them are their standard errors. Source for the first colume is Jackman (1978, Table 3). Other columns calculated by SAS PROC SYSREG, release 79.6.

and, combined with electoral turnout, it reduces the otherwise-stabilizing effect of turnout. Thus, in terms of its independent additive impact and nonadditive interactions with pre-independence political factors, ethnic dominance (or its opposite, cultural pluralism) is the single most important variable in Jackman's model.

Jackman also considered two expressly political variables, multipartyism and mass political participation. The theoretical literature reviewed by Jackman (pp. 1263-1264) offered conflicting hypotheses about the impact of these political factors on coups d'etat and other forms of political instability. Some, like Huntington (1968), see a multiplicity of political parties as destabilizing, particularly in the presence of high levels of social or political mobilization. Others, like Deutsch (1961), see the democratic pluralism of a multiparty political system as stabilizing because of the reduced scope for action such systems provide for small, conspiratorial groups of coup makers.

As Table 1 shows, Jackman found that party dominance was stabilizing, thereby supporting Huntington's hypotheses that multipartyism was destabilizing on its own and in interaction with a dominant ethnic group. Electoral turnout (political mobilization) was found to be highly stabilizing on its own. In interaction with a dominant ethnic group, its stabilizing effect is weakened and in interaction with party dominance, high turnout limits the destabilizing effects of multipartyism.

In our view, the theoretical plausibility of Jackman's model and the very strong statistical evidence in support of the theory (we report here only his most general statistical result) represent a considerable achievement in cross-national research. Under the assumption that the structural factors identified by Jackman were still operative, we decided to apply his model to our more recent and expanded data set on military coups in the majority-ruled states of Sub-Saharan Africa.

Direct Replication

First, we attempted to replicate Jackman's study exactly in order to confirm the reliability of his results. Professor Jackman kindly made available to us his coup index scores (reported in Table 2), and we independently collected data on his four explanatory variables from the same source he used, the *Black Africa Handbook* (Morrison et al., 1972). We then calculated the second regression reported in Table 1. Our results agreed with Jackman's exactly,[4] which is not always the case in comparative research.[5]

Indirect Replication

Our next theory-building step was to apply his model to our own measure of African military instability events. In our applied research (Orkand Corp., 1983), we attempted to be as comprehensive as possible and collected instances of military interventions in government and politics for all 45 currently independent, majority-ruled states of Africa south of the Sahara (the 1982 membership of the Organization of African Unity, excluding Morocco, Algeria, Tunisia, Libya, and Egypt, and also excluding minority-ruled Namibia and the Republic of South Africa).

Our Total Military Involvement Score (TMIS) was constructed exactly as were the Morrison et al. (1972) and Jackman (1978) indexes except that we included coups, attempted coups, and reported coup plots only if elements of the state's military, security, or police forces played a role in the event. We did this because our theoretical focus is on the military coup d'etat in Black Africa and not on general elite instability as studied by Morrison and Jackman. The time period covered was also as inclusive as possible, January 1, 1960 through December 31, 1982.[6] Our events were collected from four primary sources: *Index to the New York Times,* 1960—; *Keesing's Contemporary Archives,* 1960—; *Africa Research Bulletin,* 1964—; and Morrison et al., *Black Africa: A Comparative Handbook*. The Foreign Broadcast Information Service, 1960-1982, was also used to confirm or disconfirm coup plots when only one of our primary sources reported such plots. Table 2 presents the results of this measurement exercise as well as Jackman's index scores for his 30 cases.[7]

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[4] Jackman omitted decimal points from his percentage measures of social mobilization and party dominance, whereas we did not, which accounts for the different placement of the decimal points for the regression coefficients of these two variables in our two regressions. Second, our formula for calculating R^2 corrected for degrees of freedom (from Pindyck & Rubinfeld, 1981, p. 80) produced a slightly larger result than Jackman's formula.

[5] Jackman includes Ethiopia in several of his regressions but excludes this case from his final analyses because it was then an ancient kingdom without political parties. Although we find this reasoning acceptable, we were interested to see if the inclusion of Ethiopia in the analysis would make any difference, and so we did the third regression reported in Table 1. We found that adding Ethiopia to the analysis does not make much difference at all, although the statistical evidence suggests that it is something of an outlier.

[6] The only coup d'etat that occurred before 1960 in one of our 45 states was the 1958 coup in the Sudan.

When we compare our TMIS index to Jackman's 29 cases and 1960-1975 time period, we find that TMIS has a mean value of 10.55 and a standard deviation of 10.08. Thus, TMIS records less instability than Jackman's and is equally positively skewed. Since our theoretical concerns are somewhat more narrow than Jackman's and our index excludes entirely civilian coup-related events, it should record lower scores, and it is reassuring that it does.

The validity of TMIS can be tested by relating it to Jackman's index and his independent variables (Table 3); our indexes should be strongly and positively correlated, but not perfectly so, inasmuch as they measure slightly different aspects of coups d'etat. With a correlation coefficient of .911, they do correlate as expected. The validity of the TMIS can be assessed by examining the pattern of its correlations with Jackman's independent variables in comparison to the same correlations with Jackman's index. (The notion here is predictive validity (Gurr, 1972), and the correlations should be similar since by far the greatest amount of coup behavior has involved the military.) The pattern of correlations is strikingly similar for both indexes, which indicates both the reliability and validity of the TMIS index.

We then applied Jackman's model to TMIS to explore the hypothesis that the structural factors he identified are operative when the focus is entirely on military coups d'etat and when new data permit the examination of more African states and a longer time period. First, we repeated Jackman's result, and we report in Table 4, Col. 2, our first regression with TMIS in which the countries and time period are identical to Jackman's.

Our initial results were most encouraging in that the signs of all variables are identical to Jackman's and most coefficients are more than twice their standard errors. Social mobilization is not quite significant at the .05 level, but its coefficient is 1.5 times as large as its standard error and the sign is correct. Party dominance and its interaction term with turnout, *D*P*, are clearly not significantly different from zero. (Note that in Jackman's original analysis *D*P* was also not twice its standard error.) Moreover, the total regression is highly significant, and it accounts for over three-quarters of the variance in TMIS, 1960-1975.

This highly positive result suggested that Jackman's original model might well be useful when applied to our larger set of cases and longer time period (see Table 2). Because a number of Sub-Saharan states have become independent only recently—eight have achieved independence in 1975 or after—we decided to limit our analysis to all states independent since the end of 1970. Several arguments support this decision. First,

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[7] The weighting scheme for TMIS, which is the sum of all coups (5), attempted coups (3), and coup plots (1), was used in order to maximize comparability with the earlier research of Morrison et al. (1972, p. 128) and Jackman (1978). Evidence that the three types of events may be combined is contained in Table 2, where only Upper Volta is shown to have experienced coups without associated attempted coups and reported coup plots. The three forms of events would appear to represent a syndrome of military intervention in African politics.

The weights used, being based upon previous research, are conventional, perhaps even arbitrary. For example, it can be argued that successful coups are far more important than other military intervention events and should be weighted accordingly. It can also be argued that data on coup plots are unreliable and should not be part of an index. Finally, military intervention events may be multiplicatively rather than additively related. To explore these arguments we intercorrelated TMIS 1960-1982 with four new indexes defined as follows:

$A = \text{TMIS } 1960\text{-}1982$
$B = (n \text{ coups} * 10) + (n \text{ attempted coups} * 3) + (n \text{ plots})$
$C = \text{TMIS } 1960\text{-}1982 - \text{plots}$
$D = B - \text{plots}$
$E = (n \text{ plots}) * (n \text{ attempted coups})^2 * (n \text{ coups})^3$

The resulting Spearman rank order and Pearson product moment correlation coefficients among the five indexes for our 35 states were:

	A	*B*	*C*	*D*	*E*
A	1.00	.94	.97	.93	.63
B	.98	1.00	.92	.99	.83
C	.96	.98	1.00	.93	.60
D	.96	.98	.99	1.00	.82
E	.97	.97	.95	.95	1.00

The Spearman rank order coefficients below the diagonal show that the differently weighted, with or without plots, indexes *A*, *B*, *C*, and *D* are so highly intercorrelated as to be indistinguishable. The rank order of the multiplicative index, *E*, is also highly correlated with the various additive indexes, but when treated as interval data above the diagonal *E*'s correlations decline. This results from the effect on *r* of sharply varying intervals created by multiplying events for states with high levels of military involvement, e.g., for Ghana TMIS 1960-1982 = 48, *E* = 22,000 and for Uganda TMIS 1960-1982 = 37, *E* = 3,888, but for Liberia TMIS 1960-1982 = 12 and *E* = 7! We conclude that the alternative additive weighting schemes with plots included or not have nothing to offer over TMIS and that multiplicative schemes without some arbitrary transformation will produce an unacceptable degree of positive skewness in the index.

Table 2. Rank Order of 45 Sub-Saharan African States by Their Total Military Involvement Score (TMIS), 1960-1982

		Jackman's Coup Index[b]	Total Military Involvement Score[c]	Coups *N*	Attempted Coups *N*	Plots *N*
1	Ghana	32	48	5	4	11
2	Benin	46	42	6	3	3
3	Uganda	22	37	3	6	4
4	Congo	31	33	3	5	3
5	C.A.R.	7	26	3	3	2
6	Sudan	35	26	1	4	9
7	Upper Volta	12	20	4	–	–
8	Burundi	20	20	3	1	2
9	Zaire	35	20	2	1	7
10	Ethiopia	13	20	1	2	9
11	Nigeria	20	19	3	1	1
12	Madagascar		18	2	2	2
13.5	Mauritania	1	16	2	1	3
13.5	Sierra Leone	19	16	2	1	3
16	Equatorial Guinea[a]		15	1	3	1
16	Niger	9	15	1	3	1
16	Somalia	10	15	1	2	4
18	Togo	26	14	2	–	4
19	Liberia	6	12	1	–	7
20	Guinea	19	10	–	1	7
22	Chad	12	9	1	1	1
22	Guinea-Bissau[a]		9	1	1	1
22	Mali	8	9	1	–	4
24	Rwanda	10	8	1	1	–
25	Zambia	3	7	–	2	1
26.5	Comoros[a]		6	1	–	1
26.5	Seychelles[a]		6	1	–	1
28	Kenya	1	5	–	1	2
30	Gambia	0	4	–	1	1
30	Tanzania	3	4	–	1	1
30	Zimbabwe[a]		4	–	1	1
34	Angola[a]		3	–	1	–
34	Gabon	3	3	–	1	–
34	Ivory Coast	3	3	–	–	3
34	Mozambique[a]		3	–	1	–
34	Senegal	8	3	–	1	–
37.5	Malawi	3	1	–	–	1
37.5	Sao Tome & Principe[a]		1	–	–	1
42	Botswana		0	–	–	–
42	Cameroon	2	0	–	–	–
42	Cape Verde[a]		0	–	–	–
42	Djibouti[a]		0	–	–	–
42	Lesotho		0	–	–	–
42	Mauritius		0	–	–	–
42	Swaziland		0	–	–	–
				52	56	102

[a] Countries excluded from our model estimations.

[b] These data were made available by Professor Jackman, for which we thank him. No number indicates that the country was not included in Jackman's (1978) study.

[c] Where states have the same TMIS, they have been ranked on the basis of the number of successful coups they have experienced.

Table 3. Validation of the Total Military Intervention Index, 1960 to 1975 (N=29)[a]

Independent Variables	Jackman's Coup Index, 1960-1975	TMIS 1960-1975
Social mobilization (*M*)	.413 (.026)	.277 (.146)
Ethnic dominance (*C*)	.378 (.043)	.345 (.067)
Party dominance (*D*)	–.612 (.000)	–.578 (.001)
Turnout (*P*)	–.365 (.052)	–.440 (.017)
*D*P*	–.369 (.049)	–.448 (.015)
*C*D*	.091 (.639)	.038 (.847)
*C*P*	.082 (.674)	.004 (.982)

Correlation of Jackman's Index with TMIS, r = .911, f = .0001

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[a] Ethiopia is excluded. Entries are zero order Pearson product moment correlation coefficients. In parentheses are their associated probabilities in a two-tailed test of significance. Calculated by SAS PROC CORR, release 79.6.

Table 4. Regressions of Coups d'Etat, 1960 to 1975, and Total Military Intervention, 1960 to 1975 and 1960 to 1982, on Mobilization, Pluralism, Party Dominance, and Turnout

Variables	Jackman's 1978 Result[a]	TMIS 1960-1975[a]	TMIS 1960-1982[b]
Social mobilization (*M*)	.277 (.073)	10.416 (6.862)	–2.010 (7.973)
Ethnic dominance (*C*)	27.666 (7.142)	27.519 (6.726)	19.388 (11.445)
Party dominance (*D*)	–.202 (.091)	–9.703 (8.557)	–15.409 (13.027)
Turnout (*P*)	–23.047 (7.838)	–17.895 (7.380)	–29.366 (12.316)
*D*P*	.186 (.111)	10.942 (10.510)	26.625 (17.540)
*C*D*	–.317 (.104)	–35.766 (9.828)	–25.064 (17.166)
*C*P*	11.380 (4.529)	9.942 (4.264)	4.176 (7.808)
Constant	19.129 (7.326)	14.768 (6.898)	28.087 (8.614)
N	29	29	35
R^2	.843	.793	.426
$\bar{R}^2$	.791	.736	.303
F	not reported	11.51	2.86
p of F	not reported	.0001	.0229

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[a] Ethiopia is excluded.

[b] See Table 2 for a list of the countries included. Entries are the parameter estimates and below them in parentheses, their standard errors. Calculated by SAS PROC SYSREG, release 79.6.

whenever possible, all relevant states should be included in an analysis (Jackman omitted Botswana and Lesotho from his study without offering any justification). Second, time must pass before the cumulative impact of coup-facilitating conditions takes effect, a point stressed by O'Kane (1981, pp. 294-295) and Zolberg (1968a, p. 78). By selecting the end of 1970 as a cutoff point, at least 11 years of independent statehood apply to all of our cases. Third, African independence was achieved in two waves, the first from 1956 (Sudan) through 1968 (Equatorial Guinea, Mauritius, and Swaziland) when 36 states became independent, and a second wave from 1974 (Guinea-Bissau) through 1980 (Zimbabwe), when nine more states were decolonized. Thus, any year between 1969 and 1973 would produce the same partitioning of our cases.

Instead of 36 states independent before the end of 1970, we were able to study only 35, because little relevant data other than our TMIS measure were available for Equatorial Guinea (estimated 1976 population, 310,000). Although we can partition our time period in any way we desire because our coup events were collected on a daily basis, we are concerned with identifying the most general possible structural factors related to military coups in tropical Africa and therefore initially worked with our entire 1960-1982 time period. (See the Technical Note for more discussion of these design issues.) Our data set therefore adds seven years to Jackman's (1976-1982) and six Sub-Saharan African states (Botswana, Ethiopia, Lesotho, Madagascar, Mauritius, and Swaziland).

When we tried to fit Jackman's model to our expanded data set (Table 4, Col. 3), problems immediately became evident. Among Jackman's seven variables, only electoral turnout remains significant, and the entire regression accounts for only 43% of the variance, and only 30% when degrees of freedom are considered. It would appear that although Jackman's results were satisfactory for his cases and time period, they suffer a nearly complete breakdown when applied just to military coups for six more African states and seven more years.

Misspecification or Multicollinearity?

There are four possible reasons for the failure of Jackman's model to work in this instance. First, statistical problems such as multicollinearity may be a problem both in his original analysis and in our extension. Second, changes since 1975 may cause his model no longer to be appropriate. If this is the case, Jackman cannot be faulted, because structural changes during years subsequent to his research are not a valid basis for criticism of his findings. Third, the model may have broken down because of the inclusion of six new cases that Jackman could have studied but did not. Data on his independent variables were available from Morrison et al. (1972) for Botswana, Ethiopia, and Lesotho, yet he did not include these states in his final analysis.[8] We do not know why he did not include Madagascar (independent in 1960), Mauritius (1968), and Swaziland (1968) except that they were not included in his principal source of secondary data (Morrison et al., 1972). If such a change in the cases examined is the source of the poorness of fit shown in Table 4, then the external validity of Jackman's original results would be open to serious criticism. Fourth, some form of interaction among statistical problems, time period, and number of cases may be operative.

Given that both Jackman and ourselves have estimated cross-sectional statistical models, problems of autocorrelation cannot arise, but problems of multicollinearity may. Jackman (1978, p. 1269) offers evidence in the form of six bivariate correlations among his four substantive independent variables that multicollinearity "is not a problem with this model." This is wrong in at least two respects. First, the term multicollinearity "is used to denote the presence of linear relationships (or near linear relationships) among explanatory variables" (Koutsoyiannis, 1973, p. 225). Clearly, six bivariate correlations among only four explanatory variables cannot demonstrate that all possible combinations of Jackman's seven independent variables are not collinear for his data set. Second, given that three of Jackman's explanatory variables are interaction terms where one explanatory variable is multiplied by another, it should not be surprising that when all seven of Jackman's independent variables are examined for our cases and time period, as in Table 5, evidence of multicollinearity is rather strong.

The bivariate correlations we observe among the seven explanatory variables in Jackman's model as applied to our data suggest that multicollinearity may well be a problem in that the interaction terms are in some cases highly correlated with one of their components or with each other. Following Farrar and Glauber (1967, as described by Koutsoyiannis, 1973, pp. 234-241), we have regressed each explanatory variable against all other such variables in the lower part of Table 5. Computing Farrar and Glauber's *F* test for the location of multicollinearity indicates that only social mobilization is *not* collinear with the other

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[8] Jackman included Ethiopia in some of his preliminary regressions, but did not include it in his final analyses.

Table 5. Evaluation of Multicollinearity (*N*=35)[a]

Variable	*C*	*D*	*P*	D*P	C*D	C*P
Social mobilization (*M*)	.023	–.264	.156	.043	–.090	.166
Ethnic dominance (*C*)		.224	.067	.128	.906	.626
Party dominance (*D*)			.284	.488	.500	.292
Turnout (*P*)				.930	.148	.626
D*P					.285	.651
C*D						.658

R^2	Dependent	Independent[b]					
.176	*M*	*C*	*D*	*P*	D*P	C*D	C*P
.900[c]	*C*	*M*	*D*[b]	*P*	D*P	C*D[b]	C*P
.643[c]	*D*	*M*	*C*[b]	*P*	D*P[b]	C*D[b]	C*P
.914[c]	*P*	*M*	*C*	*D*	D*P[b]	C*D	C*P
.928[c]	D*P	*M*	*C*	*D*	*P*	C*D	C*P
.926[c]	C*D	*M*	*C*[b]	*D*	*P*	D*P	C*P
.767[c]	C*P	*M*	*C*	*D*	*P*	D*P	C*D

[a] See Table 2 for list of countries.

[b] These variables demonstrate collinearity with the left-hand variable in that their *t* values are greater than 2.

[c] Significant in the Farrar and Glauber (1967) test for multicollinearity. Calculated by SAS PROC CORR and SYSREG, release 79.6.

explanatory variables.[9] It would appear, then, that multicollinearity might well be a problem in fitting Jackman's model to our expanded data set.

What are the possible consequences of multicollinearity? Unfortunately, there is no firm agreement in the literature to answer this question (Koutsoyiannis, 1973, pp. 226-230; Pindyck & Rubinfeld, 1981, pp. 87-90). It would appear that although there are exceptions, such as estimated Cobb-Douglas production functions in economic research, in general, multicollinearity will increase the standard error of each regression coefficient associated with a collinear explanatory variable while at the same time the estimated coefficients remain unbiased. Multicollinearity therefore can make it difficult to determine the independent impact of collinear variables in a multiple regression. As a consequence of these inflated standard errors, theoretically important explanatory variables may appear to be insignificant and hence may be dropped from the equation, thereby introducing the theoretically far more serious problem of misspecification bias (Tufte, 1974, pp. 148-155).

Following the discussions in Pyndyck and Rubinfeld (1981, pp. 89-90) and Tufte (1974, p. 152), we did a stepwise multiple regression of this equation to see if either the estimated parameters or their standard errors changed sharply as the second through seventh explanatory variables were added to the equation. They did not. Therefore, although standard tests do suggest the presence of multicollinearity, this is not the reason for the low R^2 and the large standard errors of most variables in the last regression in Table 4, where Jackman's model is estimated for our 35 cases and 23 years. Rather, it would appear that the problem arises from our different samples, in terms of cases, time, or both.

Such an inference is supported when we examine the plot of the residuals from the last regression in Table 4 against the observed values of TMIS for 1960-1982. The plot shows a heteroscedastic pattern in which African states with low values of TMIS tend to have negative residuals (more military intervention predicted than observed) and in which states with high scores on TMIS have quite large positive residuals (considerably less predicted intervention by the military than that actually recorded). Rather than applying Weighted Least Squares or some other technique to correct for the inefficiency introduced in the estimated parameters and predicted TMIS scores caused by the presence of a heteroscedastic error term, the plot strongly suggests that the poor fit we see in Table 4 is the consequence of misspecification, specifically omitted variables (Koutsoyiannis, 1973, pp. 175-189). In other words, by adding seven years of time, six new African states, or both, Jackman's model no longer sufficies, and once we are certain of the source of this breakdown, we will have to extend his model if we are to explain theoretically Black African military coups between 1960 and 1982.

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[9] The specific variables with which a given explanatory variable is collinear can be determined by applying a *t* test to their regression coefficients. Use of this test indicates that all right-hand side variables marked "b" are collinear with the given left-hand side variable in Table 5.

Table 6 explores the possibilities. The first regression repeats the second regression of Table 4, which exactly replicated Jackman's (1978) analysis, except that TMIS is the dependent variable and not his coup index. It will be recalled that this result was quite positive and led us to believe that we would be able to use most if not all of Jackman's model in our own explanatory efforts.

The second regression in Table 6 retains Jackman's 1960-1975 time period, but increases the number of African states examined from 29 to 35. The third regression holds constant Jackman's 29 cases, but varies time to include all 23 years (1960-1982). If time is the problem and something has changed in the structure of forces causing coups in Sub-Saharan Africa since Jackman collected his data, then the third regression should fit the data less well than the second, in which time is constant but the sample of countries varies.

Unfortunately for the external validity of Jackman's model, this is not the case. The second regression is the poorest fitting of the three, explaining only 50% of the variance when degrees of freedom are considered and in which only ethnic dominance, turnout, and the *C*D* interaction term are significant. To be sure, the third regression does not fit that well either, but it does explain somewhat more variance and four of its explanatory variables are highly significant, while turnout is very nearly twice its standard error.

It would appear, then, that Jackman's well-known model breaks down when it is evaluated against our new data primarily because we have included six African states not studied by Jackman.[10] That increasing a cross-national sam-

Table 6. Regressions of Total Military Intervention on Mobilization, Pluralism, Party Dominance, and Turnout

Variables	TMIS 1960-1975[a]	TMIS 1960-1975[b]	TMIS 1960-1982[a]
Social mobilization (*M*)	10.416 (6.862)	−5.093 (5.436)	27.850 (10.477)
Ethnic dominance (*C*)	27.519 (6.726)	21.763 (7.804)	27.949 (10.270)
Party dominance (*D*)	−9.703 (8.557)	−17.556 (8.882)	−.076 (13.066)
Turnout (*P*)	−17.895 (7.380)	−22.153 (8.398)	−22.073 (11.269)
*D*P*	10.942 (10.510)	22.027 (11.959)	11.140 (16.048)
*C*D*	−35.766 (9.828)	−26.810 (11.704)	−36.786 (15.007)
*C*P*	9.942 (4.264)	2.026 (5.324)	14.438 (6.512)
Constant	14.768 (6.898)	24.290 (5.873)	8.674 (10.543)
N	29	35	29
R^2	.793	.590	.675
$\bar{R}^2$	.736	.502	.586
F	11.51	5.56	6.23
p of F	.0001	.0005	.0005

[a] Ethiopia is excluded.

[b] See Table 2 for a list of countries. Calculated by SAS PROC SYSREG, release 79.6.

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[10] In this instance, that six additional states have such an impact may have been caused by their scores on our TMIS index for 1960-1975 and for 1960-1982. Four of these states—Botswana, Lesotho, Mauritius, and Swaziland—are among the most stable in Africa and have never recorded a single military intervention event. When applied to our data, Jackman's model predicts

ple by just over 20% has such serious consequences is a most troubling finding. Countless studies published in the last two decades by sociologists and political scientists work with samples of cases, often in the same article, that vary considerably more than 20%. The stability and external validity of the findings they have reported seem to us to be questionable unless replicated as we have done in this article.

Our earlier point about residual plots indicating missing variables and the poor fit of the third regression in Table 6 suggest that a change in sample is not the only cause of the breakdown in the Jackman model; something may have changed in tropical Africa after Jackman's cutoff date, perhaps post-1973 economic deterioration caused by vastly increased energy prices and weaker demand for Black Africa's exports. A number of states, particularly the Central African Republic, Ghana, Liberia, Mauritania, Uganda, and Upper Volta experienced considerable military intervention in politics between the beginning of 1976 and the end of 1982. These states have large positive residuals when Jackman's model is used to predict their 1960-1982 TMIS scores, indicating that whatever forces were at work to produce these post-1975 interventions, they are not included in Jackman's model.

Extension

Jackman's model of African coups is a rather mainstream one, based as it is on notions of modernization and political development produced by American theorists in the 1950s and 1960s. (For an excellent recent critique of this literature, see Higgott, 1983.) These concepts, as modified to apply to Sub-Saharan Africa and with the addition of the key notion of cultural pluralism and its relationship to political instability, worked well for him, but as we have seen, did not work well for us. In general we have no theoretical problems with what Jackman included in his model, but rather with what he may have left out.

It is in this direction that our own explanatory model specification and estimation efforts turned. Specifically, Jackman did not consider two kinds of dynamics which we hypothesize should influence the location and frequency of African coups d'etat: The role and organization of African militaries, and political economy in which economic trends and patterns are seen as affecting political behavior.

Organizational Factors Affecting Military Intervention

Theorists who stress organizational factors as causes of military intervention argue that although such structural factors as cultural pluralism, social mobilization, and mass political participation may be important, they should not be considered at the expense or exclusion of variables internal to the armed forces themselves. In particular, the works of Bienen (1968, 1971), Decalo (1976), Gutteridge (1969), Welch (1970), Welch and Smith (1974), and Zolberg (1968a, b) argue that factors specific to African military organizations encourage or inhibit interventions in politics. Nordlinger's (1977, pp. 64-76) synthesis of this literature emphasizes defense of the military's collective corporate interests—particularly military budgets, organizational autonomy, and survival in competition with other government entities such as mass parties—and the failures of poorly institutionalized civilian governments to rule effectively and to promote economic growth (1977, pp. 85-91). Zolberg (1968a, b) has argued persuasively that when civilian governments shift away from political pluralism and democracy toward authoritarianism and repression, the political centrality of the military as the principal agent of repression rises, and its motivation to intervene openly on its own grows.

Although space considerations preclude a full discussion of the numerous, specifically military factors that have been suggested in the literature as affecting the chances of military intervention, these factors can for the most part be subsumed under two general categories, the military's role in society and its cohesion as an autonomous organization. The likelihood of intervention increases when the military's role in society is not professional; when civilian and military institutions, values, and roles are conflated and when defense against external threat is not the military's primary role (Huntington, 1957; Welch & Smith, 1974, p. 41). In such situations the propensity to intervene is further augmented if the military is among the "stronger" institutions in a society (as in its share of central government expenditure) and when its nationalism is overly zealous (Austin, 1966). The presence of high military cohesion increases the probability of coups, particularly when military leadership is sociologically distinct from civilian elites. A lack of cohesion

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scores considerably above zero for these states. The other two states added to the analysis, Ethiopia and Madagascar, both score above the mean value of TMIS in both the 16- and 23-year periods. Jackman's model predicts less TMIS for these two states than they have actually experienced. All six additional states are outliers when Jackman's model is applied to our 35 country data set.

facilitates more coups after the military's first intervention takes place (Welch & Smith, 1974, pp. 14-15).

In Black Africa military cohesion is problematic because of the acute cultural pluralism of most African states. This pluralism has a number of theoretical implications. First, as suggested above, the lack of cohesion caused by cultural pluralism can inhibit the military from carrying out coordinated political interventions, if that pluralism is reflected in the military itself. Second, lacking cohesion, actions as drastic as military coups can possibly cause the military's own disintegration by turning its ethnically differentiated members against one another as apparently happened in Uganda after the 1971 Amin coup. Third, to the extent that recruitment to the military creates a distinct military caste socially distant from the ruling civilian elites in society, the military can become a counter elite that under the right circumstances will seize state power.

Political Economy and Military Intervention

A second possible approach ignored by Jackman is that of political economy in which economic trends and patterns are seen as affecting political behavior such as coups d'etat. All Sub-Saharan African states are economically underdeveloped, and therefore all African governments claim that economic development is one of the major objectives against which their performance in office can be assessed, both locally and internationally. One does not have to be a vulgar Marxist to recognize that a stagnating economy in conjunction with rising unemployment and underemployment, recurrent balance-of-payments crises, and flagrant corruption create an environment in which military coups can become highly probable events.

That there may be a relationship between economic performance and military intervention has been recognized for some time in the study of African politics. Nelkin (1967, p. 231) in her early study of the mid-1960s African coups concluded that "the issues which best account for the ease of military access to power, relate to economic circumstances and their social consequences." Welch and Smith (1974, p. 26) in their subsequent survey of the literature propose that "The likelihood of military intervention rises with a perceived deterioration of economic conditions, especially if accompanied by a belief that the government cannot resolve, or is responsible for, this deterioration." In such circumstances the military coup offers some actors an apparently swift and cheap way to reverse unfavorable economic trends (O'Kane, 1981, p. 293-294).

In the face of economic deterioration, the military may be motivated to act for at least two reasons. First, the military itself can have its position threatened by worsening economic conditions. Cuts in military budgets necessitated by "austerity" programs are seldom welcome and can directly impact upon the military's lifestyle, size, equipment, and training programs. Second, if economic downturns result in social dislocation in the form of labor unrest and strikes, heightened class antagonisms, and anomic violence, this can be perceived by the military as threatening the national interest and thus prompt them to act to save the situation (Welch & Smith, 1974, p. 67).

A further interesting variant of the political economy approach ignored by Jackman is the notion of economic dependency in the context of core-periphery relations within the international political economy.[11] Contemporary approaches to comparative politics (Chilcote, 1981, 1982) now stress that states are members of a larger world system that is hierarchically organized and that in the periphery of this system (Africa), dependent economic development is often associated with weak states subject to praetorianism.

O'Kane (1981, pp. 289-293) sees the internationally determined price fluctuations of primary product commodity exports (what one of us has elsewhere called "market dependency," McGowan & Smith, 1978) as highly conducive to coups in countries that are poor and overly dependent on the export of a few such commodities. Since the onset of energy price increases and worldwide economic stagnation in 1973, the non-petroleum exporting African states have struggled to survive economically. By 1983 the U.S. State Department noted that "Africa is the only area in the world where national growth rates are often negative and where per capita food production is declining" (Bureau of Public Affairs, 1983, p. 1). After nearly ten years of economic crisis it is entirely plausible to reason that economic deterioration and dependence in Black Africa has been a major force behind the military interventions of the 1970s and 1980s.

Empirical Results of Model Extension

Based upon this reasoning, we examined a number of alternative sub-models focusing on theories of political development and social change (as used by Jackman), characteristics of African military establishments, domestic and international economic performance, competition over increasingly scarce resources given Black Africa's current economic problems, and international economic dependency. In general, none of these sub-models accounted well for TMIS 1960-1982 on its own, but each did contain variables theoretically related to coups. (For more detail see McGowan, Johnson & Slater, unpublished.) Combining the results of our sub-model analyses permitted us to specify and to estimate the regressions reported in Table 7.[12]

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[11] As discussed in, among others, Caporaso (1978), Cardoso (1972), Cardoso and Faletto (1979), Chilcote (1974), Dos Santos (1970), Lall (1975), Leys (1977), O'Brien (1975), Palma (1978), Sunkel (1972), and Wallerstein (1979).

The first regression in Table 7 includes ten explanatory variables from five theoretical traditions in the study of military involvement in African politics: characteristics of the military itself, political development and the lack thereof, social mobilization, national political economy, and international economic dependence. Each variable is highly significant, and overall the regression accounts for 91% of the variance in TMIS 1960-1982, or 87% when degrees of freedom are considered.

We find that two features of the military lead to coups and the associated events of plots and coup attempts: military cohesion and the political centrality of the military. When other factors are equal, African states whose militaries are large and ethnically homogeneous experience more military involvement in politics than states with smaller, culturally plural militaries, as predicted by Welch and Smith (1974, pp. 14-15). We find that, as Zolberg (1968a, b) argued, in African states where the military was relatively central because of its role in repression and because of its claim on state revenue, the military subsequently has become involved in politics. (Very little of TMIS 1960-1982 occurs before 1966.) Although the strength of our regression analysis refutes arguments that African coups d'etat are the result of unpredictable personal and idiosyncratic features of African military influentials (Decalo, 1976), our results do suggest strongly that variables specific to African military establishments must be considered in any search for the structural determinants of military interventions in African politics.

Two features of national politics are strongly stabilizing for our 35 African states: pre-independence electoral turnout and the degree of political pluralism around 1975. The highly significant negative coefficient of our political pluralism variable indicates that African states that have either maintained or restored some degree of party competition have considerably less military involvement than more authoritarian states. It should be noted that this finding, based on a mid-1970s variable, contradicts Jackman's (1978, p. 1271) finding that a pre-independence measure of pluralism (multipartyism) was destabilizing. The "ideal" situation regarding political parties and pluralism in today's Africa would appear to be a strong mass party (in terms of having nationwide rather than regional or ethnic-based electoral support and in terms of holding legislative seats), which also tolerates or permits minority parties to exist, to compete in elections, and to hold seats in the legislature, at least as long as they are not a threat to its continued rule (e.g., Botswana).

As Jackman did, we find that the early social mobilization of African societies is strongly related to subsequent military coups and other interventions. States whose populations were still primarily in agriculture around 1960 have experienced significantly less praetorianism than more mobilized societies. Also, early and rapid population growth in the capital city of the African state (where most coups occur) is equally destabilizing. This finding represents a troubling paradox for Africa, in that it suggests that those states where social change is happening, something both African leaders and masses presumably want, are most likely to experience generally unwanted praetorianism.

In addition to aspects of the African militaries themselves, we extend Jackman's model most in the political economic sphere. As Table 7 shows, we find that three measures of positive economic performance are all highly stabilizing and, conversely, that African states whose economies have not performed well in the 1960s and 1970s are prone to coups. States where the ratio of industrial jobs to all employment increased the most between 1960 and 1978 have been relatively stable, as have states whose economies grew in the late 1960s and whose mid-1960s export performance was superior. Finally, as O'Kane (1981) has argued, states whose commodity exports became less diversified in the early 1960s have experienced more military interventions than those whose economic or market dependency lessened in the same period.

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[12] The sources of our indicators are: 1) Military cohesion = ethnic dominance (Morrison et al., 1972, pp. 181-382) times total military manpower in thousands (USACDA, various years). 2) Military political centrality = government sanctions against opponents, 1960-1967 (Taylor & Hudson, 1972, pp. 116-123) times defense expenditure in 1965 as a percentage of general government revenue (Bornschier & Heintz, 1979, pp. 283-285). 3) Political pluralism, 1975, calculated according to the formula in Bornschier & Heintz (1979, p. 167) from data reported in *Africa South of the Sahara* (1976). 4) Pre-independence electoral turnout (Morrison et al., 1972, p. 102). 5) Percentage of the population in agriculture, 1960 (U.N., various years; World Bank, 1976, 1980, and various years). 6) Percentage increase in the population of the capital city, 1950-1960 (U.N., various years; Collier's, 1953; Commercial Atlas, 1981). 7) Difference in industrial jobs as a percentage of all jobs, 1960-1978 (World Bank, 1980, p. 138). 8) Percentage increase in GNP, 1965-1970 (Bornschier & Heintz, 1979, pp. 12-14). 9) Exports minus imports divided by GNP for 1965 (same sources as note 5). 10) Percentage increase in the commodity concentration of exports, 1960-1965 (Bornschier & Heintz, 1979, pp. 97-99; World Bank, *World Tables*, 1980). The above are our primary sources for each variable; where necessary they were supplemented by additional sources, and in a few cases the mean value of the variable was used to estimate otherwise missing data.

To summarize, we find that African states with relatively dynamic economies whose societies were not much mobilized before independence and which have maintained or restored some degree of political participation and pluralism while keeping their military forces small and nonpoliticized have been the most stable, whereas countries with the opposite set of characteristics have experienced considerable political instability in the form of military coups and associated forms of military intervention in politics.

How stable are these results? An examination of a plot of the residuals from the first regression in Table 7 against our observed TMIS 1960-1982 values indicated a homoscedastic error variance. Stepwise regression analysis of the same regression indicated great stability in the estimated values of the parameters and their standard errors, thereby suggesting that multicollinearity is not a problem in this model. (The estimated standard errors are also much less than one-half their

Table 7. Regressions of Total Military Intervention, 1960-1982 and 1970-1982, on Military Motivation, Political Pluralism, Turnout, Social Mobilization, Economic Performance, and Economic Dependency[a]

	TMIS	
Variables	1960-1982	1970-1982
Army cohesion, ca. 1970	.319 (.113)	.325 (.122)
Army political centrality, ca. 1965	.0037 (.0016)	.0006 (.0017)
Political pluralism, 1975	−76.312 (8.863)	−41.893 (9.516)
Turnout, pre-independence	−17.986 (6.521)	−12.809 (7.001)
Percentage of population in agriculture, 1960	−.653 (.095)	−.287 (.102)
Percentage of increase in the population of the capital city, 1950-1960	.043 (.008)	.038 (.009)
Absolute increase in percentage of industrial jobs, 1960-1978	−180.339 (68.881)	−163.616 (73.954)
Percentage increase in GNP, 1965-1970	−16.633 (4.228)	−14.114 (4.540)
Ratio of exports-imports to GNP, 1965	−.763 (.137)	−.306 (.147)
Percentage increase in export commodity concentration, 1960-1965	.150 (.034)	.134 (.037)
Constant	79.708 (10.115)	42.082 (10.860)
N	35	35
R^2	.906	.765
$\bar{R}^2$	.872	.680
F	23.24	7.80
p of F	.0001	.0001

[a]See Table 2 for a list of countries. Calculated by SAS PROC SYSREG, release 79.6.

coefficients.) Since all but our measures of political pluralism and industrial-employment creation date from 1970 or before, we partitioned our TMIS index and did a second regression reported in Table 7 where only TMIS 1970-1982 is dependent.[13] Both the political centrality of the military around 1965 and pre-independence electoral turnout cease to be significant, thereby indicating that they are mainly related to 1960s military interventions. The remaining eight explanatory variables retain their signs and statistical significance, which indicates the stability of our primary results.

Conclusion

Paradoxically, our findings contradict, support, and extend Jackman's (1978) study. The contradiction is in the fact that the addition of six new countries and seven years of behavior to the analysis causes a rather complete breakdown of his well-known model. But in extending his analysis and producing a theoretically relevant and statistically robust model of African military coups d'etat, we have supported his basic conclusion that *"instability of this kind is not random with respect to political and social structure"* (1978, p. 1273, Jackman's emphasis). Although it is true in a phenomenological sense that every plot, coup, and attempted coup are the consequences of the motivations and decisions of small groups of men acting in secret, both Jackman's and our findings contradict those who see African military intervention as an unpredictable, random phenomenon (Decalo, 1976, p. 22; Zolberg, 1968b, p. 71).

Rather, it would appear that an identifiable structure of forces encourages or discourages coup makers. Agreeing with Jackman, we see social mobilization as favoring coup decisions, whereas political participation lessens them. Where we extend Jackman and thereby shed new light on the role of the military in African politics is to introduce as important explanatory variables political pluralism and domestic and international economic performance. As peripheral members of the world system, Sub-Saharan African states are generally weak and subject to praetorianism. It would appear that in states where influentials have internalized the rules of the capitalist world economy and thereby coped relatively well with the very harsh international economic environment of the last 10 years, these states have lessened their peripherality to a degree, strengthened their civilian structures somewhat, and experienced less military interventionism than states whose influentials have not coped as well (Wallerstein, 1979, pp. 66-94).[14]

Technical Note

This article has shown how a small change in the sample of cases and time period studied can have great effects on the substantive results of analysis. It thus raises some interesting technical questions for quantitative, cross-national research. First, what should be the space-scope or universe of cases examined in such studies? Clearly, the specific research question addressed should be the basic determinant of the sample used in any cross-national study. Since coups d'etat involve the extralegal seizure of state power, the cases should be (nation) states. But which states? Again, because our focus is on coups d'etat, we would argue that the sample studied should not comprise all states in a given year such as 1970 because such a sample would include many states of the First and Second Worlds where coups are not a theoretical or practical problem. All Third World states, then? In principle, we would answer yes, if one's research interest is the coup d'etat. Since our stated focus is the African military coup d'etat, we have excluded Latin American, Middle Eastern, and Asian Third World states from our study. This may be justified, not only because of our interest in replicating Jackman's (1978) earlier research, but also because the states of Black Africa share a continuing record of coup activity into the 1980s.

Our space-scope was thus the 50 members of the Organization of African Unity plus Namibia and the Republic of South Africa. We excluded the five North African states from Morocco through Egypt because we see them as belonging to the Middle Eastern, Arab Islamic culture complex whose military patterns differ from those of Black Africa. As long as RSA and Namibia remain under minority rule, we believe that they too should be excluded from a study such as ours.

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[13] We thank Professor William R. Thompson of the Claremont Graduate School for this useful suggestion that we partition TMIS 1960-1982 in this fashion in order to explore the stability of our results.

[14] We intend this conclusion to be descriptive, not prescriptive. Relatively successful adaptation to the constraints of the capitalist international political economy, including continuing military dependence on Western states, may have worked in the 1970s and early 1980s. There is no guarantee that it will continue to work in the future. As Pachter (1982) recently argued, relatively strong single party socialist regimes whose militaries are *not* Western dependent, such as Guinea, Mozambique, and Tanzania, have so far also avoided military coups in spite of their rather dismal economic performances. Also, a coup in early April 1984 in Guinea weakens her argument considerably.

Thus, left with 45 politically independent, majority-ruled states of Black Africa, we further reduced our sample to the 36 that have been independent since 1970 in order to permit sufficient time to pass for the presumed effects of structural forces to appear. Because of a lack of data for our independent variables, we were also forced to exclude Equatorial Guinea, thus ending up with 35 states in our sample.

We would make four points about the space-scope of this article and in cross-national research in general. First, given our theoretical focus, the sample of states is appropriate. Second, samples in quantitative cross-national research are always somewhat arbitrary and are viewed as more or less valid to the extent that scholars accept or reject such classificatory terms as Latin America, Black Africa, Third World, or Periphery (Dogan & Pelassy, 1984; Przeworski & Teune, 1970). Third, how the sample of cases was arrived at should always be explained, hopefully in theoretical terms and not just because of data availability from among, for example, all "Third World states." Finally, our data problems with Equatorial Guinea suggest that cross-national researchers would be well advised to adopt as a rule-of-thumb the World Bank guideline of excluding from samples the ministates of our world with populations of less than one million (World Bank, 1980).

A second question of some importance is the definition of time-scope in quantitative cross-national research. In this article we have worked with three cross-sections of varying lengths, 1960-1975, 1960-1982, and 1970-1982. We started with 1960 because it was only in that year that a sizable number of Black African states achieved political independence. Our ending date was simply as recent as possible, given the research resources available to us. It was not necessarily the case, however, that we had to work with cross-sections aggregating rather long time periods. Given that our military intervention data were collected on a daily basis, we could have worked with annual, half-yearly, or even quarterly time series. A second alternative was to work with several cross sections, thereby utilizing a pooled time series—cross-sectional design as in Zuk and Thompson's (1982) recent study of the spending patterns of military regimes.

At the present time, genuine time-series analysis of the dynamics of military intervention in African politics is precluded by the absence of annual or quarterly data for most African countries on more than a few basic economic variables. There is also the as-yet-unaddressed problem of how to score the dependent variable, in that coups, plots, and attempted coups are irregular events and a time series of such events would be mainly zeros recording no events in most time periods. Nevertheless, as Brunner and Liepelt (1972) showed, inferences about dynamic phenomena such as coups d'etat are best based upon dynamic research designs and data analysis strategies.

Given current data constraints and the dubious external validity of studying but a few states via true time-series analysis, the pooled time-series—cross-sectional treatment of time-scope would appear to be the most promising future approach in quantitative cross-national research (Pindyck & Rubinfeld, 1981, pp. 252-261). Thus, instead of estimating a model for 35 countries at one point in time ($N=35$), we could break our time-scope of 23 years into, say, four roughly equal time periods and estimate the model against a data set of 35 states and four time periods ($N=140$). The degree to which the model's parameters are or are not stable with respect to time would reveal actual structural persistence or change as may have occurred in our data after 1975. A further refinement would be to specify a simultaneous set of equations to be estimated against a pooled time-series—cross-sectional data set.

There is a danger here of techniques overtaking the quality of the data for our independent variables. There are also the problems of weak theory in the area of comparative military studies making it difficult to specify systems of equations and the essentially arbitrary nature of the time periods in pooled time-series—cross-sectional designs; since data are gathered for each time period before analysis and therefore, without strong theory, the periods selected are very likely to be conventional ones, such as five years or with small numbers of states, one year long. Nevertheless, because our article is only the latest and not the last word on these matters, we hope our discussion of these issues will stimulate others to incorporate the considerations raised in this Technical Note in their cross-national research on military coups and other important political phenomena.

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Changing National Images: International Public Relations and Media Agenda Setting

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Research within the agenda-setting framework has generally ignored the potential influence of purposive efforts by external actors (those outside the political system) to manipulate media coverage related to their interests. The present study uses interrupted time-series analysis to examine one such set of manipulative efforts, those undertaken by professional public relations consultants to influence the images of foreign nations as portrayed in the United States press. Data represent New York Times *coverage of six nations that signed public relations contracts with American firms during the period from 1974 to 1978, and one nation that expressly rejected such a contract. The analysis identifies consistent patterns of improvement along two primary dimensions of national image, visibility and valence, which are associated in time with the public relations contracts.*

> None of the regular ways you report in the United States apply. You can't run to people on the streets and stick a camera in their faces. You don't speak their language, and if they don't like you, then they shoot you. It's very, very frustrating to work in places like that [Lebanon]. . . . The normal ways in which you pursue stories just go totally out the window.
>
> It is difficult to go beyond the information that is chewed up and dispensed to you every day. It's maddening when you have to rely on the [official] spokesmen, and you strongly suspect that they're not playing it down the middle. . . . For most of us, we're constantly going to be thrown into situations under enormous time pressure, under enormous pressure to produce. . . . Those are problems I really don't see a solution to. (Josh Mankiewicz, ABC News foreign correspondent, cited in Weisman, 1983)

> Even for senior officials, regarding dispatches from newspaper correspondents abroad can provide a useful corrective to the intelligence reports and diplomatic cables that cross their desks every day; indeed, a good many of those reports and cables are themselves based on analysis of stories in the foreign press. For lesser officials and for legislators not privy to the full flow of internal documents, the press is often the main source of up-to-date information on goings-on in other capitals.
>
> If officials in one government rely on foreign correspondence as a source of information, then officials in another government have some incentives to use the news to transmit information to them. (Leon V. Sigal, 1973)

> Everybody [uses reports from the newspapers to measure the decline of political violence in El Salvador]. That's basically the general source. (Thomas Enders, at the time Assistant Secretary of State for Inter-American Affairs, on *The MacNeil-Lehrer Report,* January 21, 1983)

One of the most interesting trends in political image-making in recent years has been the growing use of professional public relations consultants by national governments.[1] This trend has been especially pronounced in the United States, where such assistance is readily available and where, the assumption seems to be, an improved national image can be translated into more concrete gains.

Although a few such efforts had been undertaken earlier, the 1970s saw a veritable explosion of lobbying and public information campaigns on behalf of foreign governments. Foreign agent registration records of the Department of Justice show, for example, that between 1974 and 1978 alone some 25 nations contracted with American public relations firms for assistance. Although the

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[1] Although the present analysis focuses on use of public relations by governments to influence their images in another country, similar practices are emerging even within countries. The British government, for example, recently contracted with an American public relations firm to generate support among its own citizens for the deployment of intermediate-range nuclear missiles ("U.K. plans ads to support deployment of missiles," 1983).

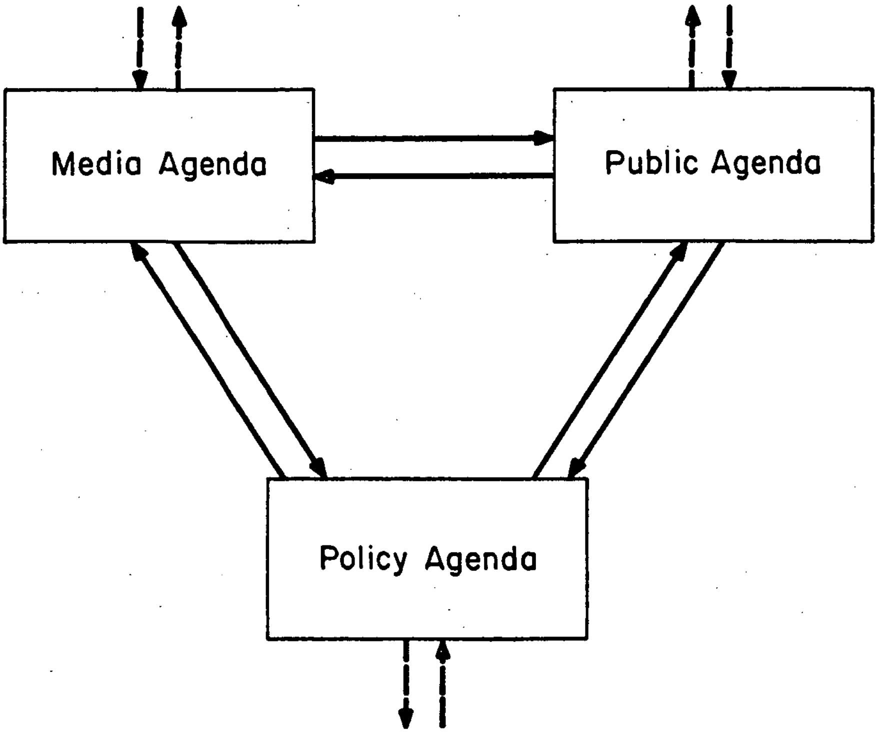
goals of these campaigns vary, one of the most common is to improve the client nation's image in the American press.[2] In the present analysis we shall examine the impact that such public relations efforts have had on news coverage of the contracting nations in one of the most influential newspapers in the United States, the *New York Times.*

The Agenda-Setting Framework: An Overview

Social scientists have long recognized the importance of images and symbols in public information campaigns. Most recently this recognition has centered on the notion of agenda setting and its implications for the exercise of political control. As indicated in Figure 1, this conceptualization has emphasized the structuring and interactions among three distinct agendas, those of the mass media, the public, and policymakers. In general, the policy agenda (often referred to as the "formal" agenda) has been described as specific, decision-oriented, detailed, and based primarily on direct or experiential information that is relatively independent of the relationships illustrated in Figure 1 (Cobb & Elder, 1972, especially pp. 82-93). The public agenda has been seen as more general, more ambiguous or symbolic in character, and more likely to be based on indirect (mediated) information for most issues (Cobb & Elder, 1972; Miller, Goldenberg, & Ebring, 1979; Robinson & Hefner, 1968; cf. Kelman & Ezekiel, 1970). The media agenda has been pictured as actively interpretive, market oriented, and based on a mixture of direct (reporter as observer) and indirect (reporter as conduit) information (Bennett, 1983; Epstein, 1973; Sigal, 1973).

Agenda setting, then, is the process by which items enter upon or pass between these respective agendas, and its study emphasizes the ways in which each interacts with the others and with their mutual external environment. Indeed, the most recent work within this framework has been the most generic in this regard. Graber (1984), for example, has demonstrated the psychological processes by which members of a news audience select

Figure 1. The Agenda-Setting System



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[2] Each of five firms responding to our request for information about the services they provide for such clients reported that these informational efforts were central to their activities.

and internalize information from the media. Page and Shapiro (1983) have demonstrated leading effects of the public agenda on the policy agenda for both foreign and domestic issues, and Iyengar, Peters, and Kinder (1982) have shown that media coverage of issues influences their presence and importance on the public agenda. Cook et al. (1983) have provided preliminary evidence that the mass media exercise direct influence on policymakers and others influential in the policy process. In addition, Cook (1982) has examined the ways in which issues disappear from the agenda, an aspect of the process that had previously been overlooked.

Much of the research on agenda setting, and especially that exploring linkages between the media and public agendas, has focused on political campaigning (Patterson & McClure, 1976; Shaw & McCombs, 1977), and indeed, the important, early work on media agenda setting (McCombs & Shaw, 1972) was undertaken in this context. Because the campaign represents an empirically distinguishable set of communications which are overtly persuasive in intent, are bounded by identifiable time points, and have an audience (potential voters) that has been presensitized to both their availability and their significance, campaign messages represent a target of opportunity for political communication researchers. We believe, however, that for precisely these same reasons campaign communication is atypical not only of political communication generally, but of communication with a persuasive intent in particular. Rather, we believe that most such communication is more subtle in style, more diffused through time, less readily recognizable to its audience, and less clearly tied to judgmental or behavioral expectations. Such communication often takes the form less of generating a set of messages than of directing, facilitating, inhibiting, and generally controlling the overall flow of information.

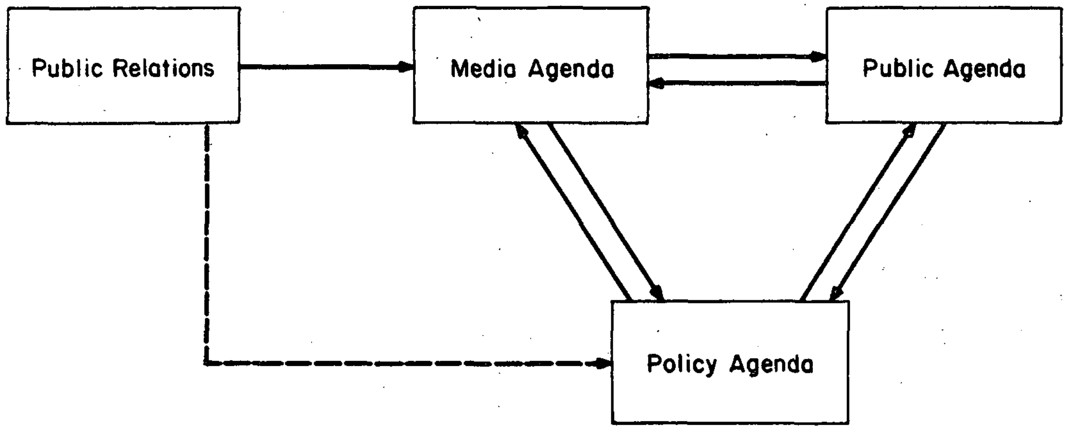
The balance of research into agenda setting, that which examines noncampaign communication, is often characterized by a similarly limiting assumption, namely that the external environment in which the process operates is benign, or at least neutral. Public officials and reporters may try to manipulate one another (Sigal, 1973), the media may attune their coverage and editorial criteria to their audience (Bennett, 1983) or their corporate well-being (Epstein, 1973), and symbolic outputs may substitute for substantive policymaking (Elder & Cobb, 1983), but all of these are viewed merely as regular transactions within and among the media, the public, and the policymakers. The generation of information from outside, that which the various actors gather independently of one another, is regarded, if the absence of scholarly attention is an indicator, as a controlled exercise. The information is there for the taking, and those who supply it are passive sources. Here, too, we believe that the present conceptualization of agenda setting falls short in that it directs our attention away from forces that may well be shaping the flow of information.

It is in this context that we have undertaken the present research, which focuses on systematic attempts by external actors to manipulate the media agenda, and through it the public and policy agendas. These efforts are illustrated in Figure 2. In particular, we shall examine the effectiveness of structured attempts by foreign nations to manipulate the ways in which they are portrayed in the United States press. (The broken line in Figure 2 represents direct lobbying efforts which are often associated with, but for our purposes will remain analytically distinct from, the attempt to control the flow of information to and through the media.)

Such efforts to manipulate media coverage of foreign actors, events and policy issues are relatively likely to succeed for several reasons. To begin with, foreign affairs are generally unobtrusive, i.e., the public is unlikely to have any direct experience with them (Eyal, 1980). In the absence of direct personal contact, individuals' images of the actors and events on the international scene will be heavily, and unavoidably, media dependent. (This is a logical extension of the argument raised by Palmgreen and Clarke, 1977.) Second, because they are limited in their ability and inclination to devote staff and resources to foreign affairs (and because audience interest in such matters is relatively low in any event), American media may be especially vulnerable to manipulation of their international coverage (Becker, 1977). Third, information gathering about such issues is difficult even for public officials (Cohen, 1963; Sigal, 1973), and as a consequence policymakers themselves may be forced to rely in some measure on media reports for information about events or settings. Indeed, even the direct reports on which foreign policymakers most rely are often based to some extent on local or international media coverage and interpretations. The protagonists in these manipulative efforts, foreign governments, clearly lie outside the system of agenda exchanges, but their interest in its operation is nevertheless keen. Media coverage has long been used by such external actors both to reach interested publics in the United States and to provide an informal communication channel to United States policymakers themselves (Davison, 1974; Sigal, 1973).

This combination of motive and opportunity places foreign governments in a potentially very favorable position vis-à-vis the dynamics of

Figure 2. Public Relations and Agenda Setting



agenda setting. For one thing, they are precisely situated to catch the system with its figurative guard down, in effect to sneak past the gatekeepers by entering the agenda-setting process at a point where the independent information-gathering powers of all three primary actors are most limited. Yet at the same time, they can take full advantage of the operating propensities of that system. As Page and Shapiro (1983), Graber (1983) and others have demonstrated, the internal forces of agenda setting function in foreign affairs much as they do in the domestic arena. The process works in those instances where it is vulnerable to such external intervention just as it does elsewhere. Thus with its emphasis on public relations interventions in behalf of foreign governments, the present research examines a situation in which the internal operations of the agenda system are likely to be functioning in a normal fashion, but in which the external environment may be, in a sense, relatively more proactive (and more effectively so) than is generally recognized.

Agenda Manipulation and Image Change

For the present, we shall confine our attention to the first stage of these attempts at agenda manipulation, the influencing to a nation's advantage of its coverage in the United States press. In general, long-term patterns of foreign news coverage have been found to vary in association with the levels of trade and telecommunications traffic between the United States and the nation in question, that nation's overall status or importance in the world, the population of the country, the apparent national interest of the United States, the occurrence of crises, and the problems posed by foreign censors (Charles, Shore, & Todd, 1979; Lent, 1977; Peterson, 1981). With the possible exception of crises, we see these factors as establishing a baseline for the overall level and type of coverage any given nation is likely to receive. This baseline is then subjected to trends in the long term as these factors undergo secular change, and to disturbances in the short term as events of varying character and duration occur. All of the nations we have studied were, at the times in question, experiencing political difficulties that undoubtedly contributed to the decision of each government to attempt to manipulate the media agenda. It is important to note, however, that by and large these difficulties continued unabated throughout the entire period of our analysis in each case, and that there was no apparent, consistent structural or temporal pattern of issue or event maturation associated in time with the respective decisions of these various countries to undertake manipulative behaviors.

The particular behavior we examine is the signing by various governments of contracts with American public relations firms for the sole or partial purpose of influencing the flow of information about that country to or within the United States. Mendelsohn (1973) has demonstrated that when used with some sophistication, information campaigns can have an impact on public perceptions, and, more recently, Merritt (1980) has suggested that governments are becoming sophisticated communicators by learning to structure informational settings to facilitate persuasion rather than by relying on more heavy-handed propaganda techniques. Marcum (1969) has documented such an instance in which Portugal, at the time fighting to maintain control of Angola, successfully employed an American public relations firm to guide American attitudes into opposing the struggle for national liberation. And Davis (1977) has provided a comprehensive longitudinal case study of the use of public relations in behalf of Nigeria and its various regional and economic interests that focuses directly on the terms and conditions of the contracts themselves.

As one might expect, the preparation of press kits, direct mailings, newsletters, and brochures is prominent among the services provided under public relations contracts. More important, however, public relations firms also routinely counsel their international clients' embassy personnel on how to talk about such problems as terrorism or human rights, schedule and conduct field trips for the press, organize visits with editors and lunches with business groups, conduct financial seminars dealing with opportunities offered by the countries, use their own contacts to help representatives of the client states develop personal relationships with officials of the United States government, and provide a variety of advice about specific policies or approaches to policy that a client government might adopt to improve its image. Perhaps one of the most important services such firms offer is to provide easier access to officials of the government in question for American reporters (Cooney, 1979; Davis, 1977).

Nimmo and Savage (1976, p. 8) have defined an image as "a human construct imposed on an array of perceived attributes projected by an object, event, or person" which is subject to influence by messages issued by some external actor. This definition provides a useful starting point for the present analysis because it allows us to distinguish between the perceived image (which Nimmo and Savage emphasize and which will provide the ultimate measure of public relations impacts on the public agenda) and the projected image, the messages through which imagemakers attempt to generate or change public perceptions. In this article we shall focus on the latter.

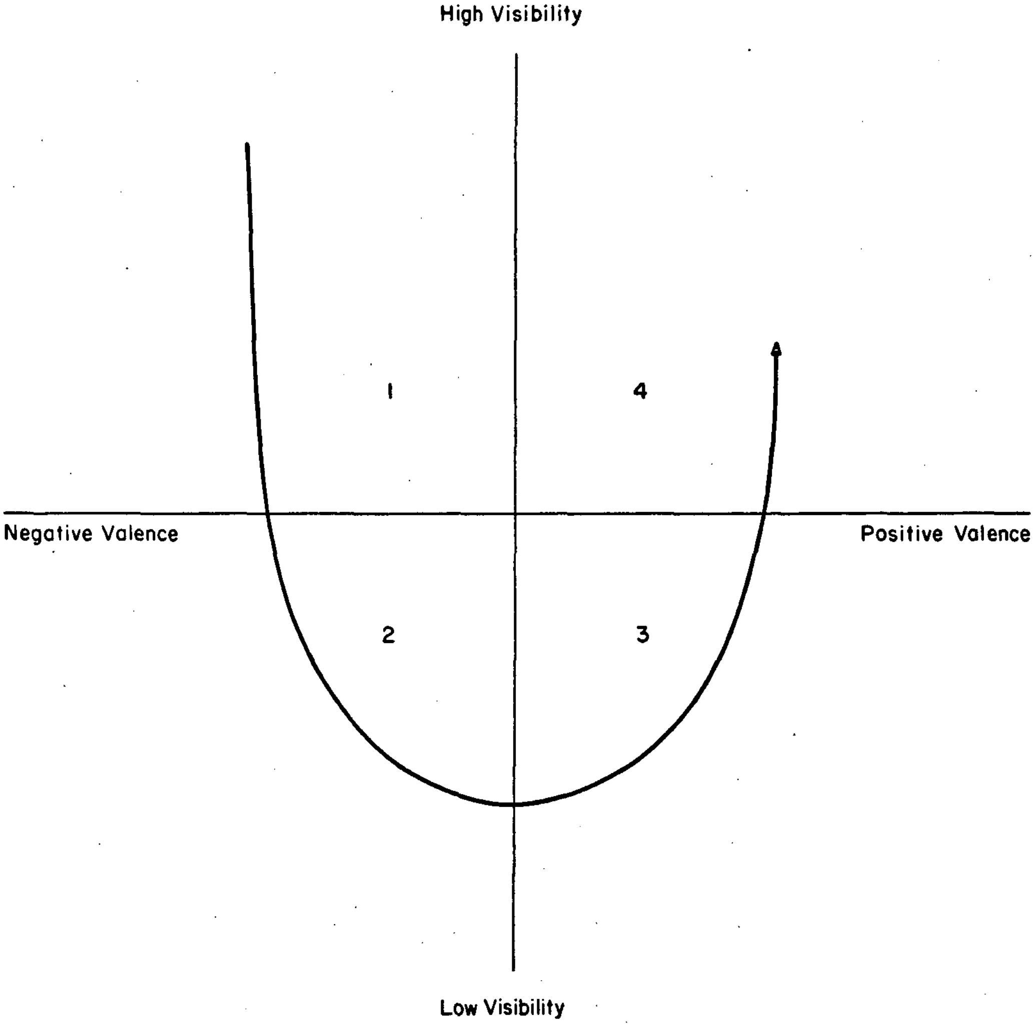
Two aspects of national images as portrayed in the United States press are of particular importance. The first, visibility, refers to the amount of media coverage that the country receives. The second, valence, refers to the degree to which the content that is available reflects either favorably or unfavorably on the country. The relationship between these primary components of national image is illustrated in Figure 3.

Quadrant 1 in Figure 3 represents countries with negative images that receive relatively heavy media attention. This type of coverage is likely to be accompanied by substantial public awareness of the country in question and of its projected image (Benton & Frazier, 1976). In such circumstances, overt efforts to portray a country in a positive light are likely to be rejected, either by the media or the public, as propaganda in the most pejorative sense (Merritt, 1980; Wolfsfeld, 1983). Hence effective public relations on behalf of such countries should probably take the form of reducing visibility, perhaps by controlling access to events and information or, alternatively, by altering or reducing the amount of government-generated information. But whatever the tactic, constructive image change here should be centered on the visibility dimension.

Quadrant 2 represents countries whose images may be quite negative, but whose visibility is sufficiently low that the public is unlikely to know or to think much about them. This is similar to what Krugman (1965) has described as a condition conducive to low-involvement learning in which an audience is particularly susceptible to certain types of persuasive communication that it might otherwise find unpalatable, and also corresponds to the type of setting that Wolfsfeld (1983) sees as the most conducive to successful persuasion through international propaganda. Thus for countries in quadrant 2, and especially for those whose visibility in the media is the lowest, effective public relations should emphasize more substantive forms of image enhancement rather than mere withdrawal from the agenda. Put another way, the lower a country's initial visibility, the more the target of any efforts at constructive image change should move from the visibility to the valence dimension. Although not concerned directly with the valence of policy images, Eyal (1980) has suggested that most foreign policy issues would have relatively low visibility and low levels of associated public awareness, and in fact the images of all but one of the countries we have analyzed lay within quadrant 2 and could be so characterized.

Passing between quadrants 2 and 3, one crosses the boundary between what Cohen (1973) has termed reactive manipulation (putting out fires) and active manipulation (positive image development). And indeed quadrants 3 and 4 are to an extent mirror images of their negatively valenced counterparts. Because countries in both quadrants start from a more favored position, the focus of their public relations is more likely to be on increasing visibility and further enhancing valence in order to reinforce a desired image. McGuire (1964) has suggested for such purposes a strategy of inoculation against negative slippage which uses such persuasive themes and devices as the forging of links between the image in question and various valued goals of the public or, alternatively, encouraging public expressions of acceptance of the image. None of the nations in this study could be characterized as having had a

Figure 3. Dimensions of National Image



positive image either at the time the contract was signed or during the preceding year. This should not be surprising, because countries with more positive images are less likely to perceive a need for public relations counseling. Accordingly, our analysis will focus on countries in the first two quadrants.

Taken together, the situation-specific strategies we have described above suggest that an informed approach to public relations activity on behalf of foreign nations is likely to vary systematically depending upon the initial position of the client state on the two dimensions of the informational setting represented in Figure 3; the goals and associated tactics of such efforts are summarized by movement along the path indicated in the figure. The question at hand is whether actual public relations campaigns produce effects that are consistent with the pattern suggested here. Evidence of such a pattern may well indicate the effective exercise of agenda manipulation.

Analysis of News Coverage

Our analysis centers on coverage of six nations—the Republic of Korea, the Philippines, Yugoslavia, Argentina, Indonesia, and Rhodesia—in the *New York Times*, each during a two-year period associated with a professional public relations effort, and of a seventh nation, Mexico, which in December 1975 expressly declined to contract for such services.[3] The contracting nations were selected to reflect a variety of factors including differences in population, location, and historical and political circumstance, as well as constellations of differing visibility and valence characteristics that are useful for examining the questions we have raised. Mexico, which faced image problems quite similar to those of the other nations, represents the only specific control we have been able to identify during the period in question in that it is the only country for which we have been able to document a specific decision at a specific point in time *not* to seek professional public relations counseling from an American firm. The *New York Times* was selected because it is the most widely read newspaper among elites both within and outside of government (Weiss, 1974) and among the most widely cited by policymakers (Grau, 1976), it has been shown to have a strong agenda-setting effect on public opinion (Winter & Eyal, 1981), it carries a higher volume of foreign news than other major U.S. newspapers (Semmel, 1976), and it is often used as a source of events data by researchers (Hopple, 1982). In addition, precisely because it is atypical of the U.S. press in that it devotes more of its own space and resources to foreign news coverage and is therefore among the most independent of U.S. newspapers in its information gathering, the *New York Times* represents not only a primary target for public relations efforts, but an acid test of their effectiveness as well.

From the mandatory foreign agent registration records of the Justice Department, we first identified those countries that had hired American public relations consultants during the period from 1974 to 1978 and the month in which each contract was signed.[4] For purposes of analysis, we defined the first day of the following month to be the point of intervention in each instance. For Mexico, the same treatment was applied to the date of the decision to forgo a contract. We then determined for each country a unique 24-month period including the 12 months immediately before and after the intervention, and gathered serial data on several qualitative and quantitative indicators of the news coverage of that country in the *New York Times*. Our data were drawn from the summaries of all articles provided in the *New York Times Index*.[5]

Treating each insertion in the index as a unit of analysis and the aggregates for each month as a single observation, we coded news coverage of each country on several variables including (but not limited to) the number of articles pertaining to the country and the number of such articles which could be judged as portraying either a positive or a negative image. Negative references included any mention of decline, weakness, poverty, liabilities, lack of progress, instability, or unreliability on the part of a given country. Positive references included such points of discussion as a country's progress, advances, resources, assets, strengths, continuity, stability, reliability, or dependability.[6]

The actual public relations advice, of course, was provided in private meetings to which we were not privy, and although we have been able to obtain some materials developed by public relations firms for their international clients, we can do little more than guess at the specifics of what suggestions were made, which were accepted, and how they were implemented. In effect, although the intervention is a matter of public record, its precise nature is, for the present, something of a black box. What we can observe, however, is any change in the behavior or news coverage of a given client state associated in time with the contract to determine the effects of the public relations effort and to support speculation about its elements. Thus our primary concern here is less with the components of the intervention than with its effects.

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[3] Analysis periods were as follows: Republic of Korea, April 1977 through March 1979; Philippines, February 1977 through January 1979; Yugoslavia, December 1976 through November 1978; Argentina, June 1975 through May 1977; Indonesia, July 1976 through June 1978; Rhodesia, December 1975 through November 1977; and Mexico, January 1975 through December 1976.

[4] The initial population of 25 contracting countries was reduced to 15 by eliminating those for which the overall amount of *New York Times* coverage was too limited to permit meaningful statistical analysis. To the best of our knowledge, none of the contracts analyzed here represented an extension of an earlier agreement.

[5] Previous research has shown that index entries even substantially more abbreviated than those in the *New York Times Index* provide a useful indication of the content of the items in question (Ptacek et al., 1975), and others have used the *New York Times Index* itself (e.g., Azar et al., 1972). Graber (1983) has found in a study of domestic campaign coverage that coding the index as opposed to the full *New York Times* text tends to understate the amount of coverage of foreign affairs issues, but since we are dealing here primarily with identifying objects and general characteristics of coverage rather than specific assertions, and since, in any event, Graber's data suggest no reason to suspect systematic differences in coverage between periods, we do not believe the variations she has observed will materially bear on the present data except to warn that we may actually be understating public relations effects by using relatively insensitive measures.

[6] The coefficients of intercoder reliability (in this case Pearson's *r* measuring the correlation between two coders' scoring of the same text) for number of articles and number of negative and positive references were 1.0, .90, and .84.

At the anecdotal level, several patterns of behavior can be noted among the countries under review which are associated in time with, and seem to reflect, the public relations effort. These include such devices as visits of the head of state to the United States or the issuance of an invitation for the American president or vice-president to visit a given country, ending of political arrests followed by one or more releases or reductions in the sentences of either leading dissidents or masses of political prisoners, government-organized field trips for journalists to locations which call attention to the government's own agenda or point of view, cosmetic redistributions of power within the country, initiation of or recommitment to extended international negotiations, the scheduling of elections, establishment of overseas information offices, and the dissemination of particular stories or symbols that are clearly designed to appeal to certain constituencies in the United States or elsewhere. A brief review of the circumstances and events in each of our countries will help to illustrate the point.

Republic of Korea. The analysis period includes the two years beginning in April 1977. During this period, the United States gave prolonged consideration to a reduction of its forces stationed in the country, the government engaged in a series of disagreements with the Roman Catholic church, the United States was accused of spying on South Korean leaders, Korean bribery of U.S. congressmen was under investigation, and opposition leader Kim Dae Jong was placed under arrest after disappearing from Japan. Among the anecdotal evidence of public relations activity during the postintervention period were a visit by Soviet leader Leonid Brezhnev (a reminder of the potential dangers of an American military withdrawal), resumption of on-again-off-again negotiations with North Korea over reunification, a meeting between Presidents Park and Carter, continuation of an earlier pattern of prisoner releases, a U.S. Senate study tour of the country, and the development of a newly discovered North Korean infiltration tunnel into a publicized tourist attraction. In addition, the *New York Times* reported some two months after the intervention that the Korean government had become noticeably more sensitive to its image abroad.

The Philippines. The analysis period began in February 1977. Events during this period included a continuing Moslem rebellion in the south, imposition of martial law, detention of journalists, discussion of continuing leases for U.S. military bases, and a series of incidents of political violence. Postintervention events in this instance included a visit by Vice-President Mondale, formation of a new pro-Marcos political party and holding of elections by President Marcos, a promise that increased power would be granted to the legislature, the reluctant freeing of a leading dissident and a series of pardons, amnesties and releases of political prisoners, signals of improving relations with China (again a reminder of the country's strategic importance) including a Chinese visit to Manila, and the highly publicized discovery of a primitive tribe in a remote area (despite some indications that the tribe had actually been discovered earlier). The public relations contract itself briefly surfaced as an issue when, in a rare opportunity to appear on television during the election campaign, President Marcos's opponent charged that the government had hired an American public relations firm with CIA connections.

Yugoslavia. Here the analysis period began in 1976. During the two years in question, Yugoslav agents and activities overseas were subjected to several attacks by Croatian dissidents, President Tito travelled extensively (France, Portugal, Algeria, China, the Soviet Union, North Korea, and, after the intervention, the United States) and was visited by heads of state and by Vice-President Mondale, the government negotiated a series of international loans and trade arrangements, and the nation prepared for the transition of power after Tito's death. Anecdotal evidence relating to the public relations campaign here is more difficult to identify, but may include a series of articles on the stabilizing role to be played by the military during the transition.

Argentina. The analysis here covers the period beginning in June 1975. During the two years under review, inflation, strikes, and government deficits plagued the Argentine economy, terrorists and guerillas threatened the stability of the Peron regime and the President herself took a leave of absence to avert a military coup, an investigation was launched into charges of government corruption, Peron announced that she would not seek reelection and later withstood an attempt at impeachment, cabinet changes were regular events, Peron was arrested in a military takeover (month 10) and Videla was elected president, Videla narrowly escaped two assassination attempts, and the government was repeatedly criticized for its human rights policies. Post-intervention activities of the military government included the release of political prisoners, reopening of a leading university, imposition of a ban on anti-Semitic and pro-Nazi literature, frequent announcements that foreign debt and other economic problems were being resolved, replacement of several generals in the government with moderate civilians, and calls by President Videla for a rapid return to democracy.

Indonesia. Here the review period commenced in July 1976. Major events included bribery scandals involving international oil companies and the Indonesian military, a foiled coup attempt, the banning of *Newsweek*, charges of human rights violations, an electoral victory by President Suharto, reductions in U.S. military assistance, a kidnapping incident in the Netherlands involving an Indonesian ethnic group (South Moluccans), and a crackdown on the press. During the post-intervention period the government granted amnesty to and released political dissidents, opened talks with the United States on its human rights policies, hosted a visit by Walter Mondale, reopened the nation's newspapers (the ban, too, came after the intervention), and began talks on a new constitution.

Rhodesia. The analysis period, commencing in December 1975, opened with the start of constitutional talks aimed at shifting from white minority to black majority rule, a process of negotiation that continued with greater or lesser intensity throughout until, in the final month of observation, the Smith government accepted majority rule in principle. During this time the country was wracked by guerrilla warfare, engaged in a series of border clashes with the so-called frontline black nations, and was regularly maligned by a variety of foreign leaders. Two months before the intervention there occurred what seemed to be a breakthrough in the negotiations, but at least in the public record this proved to be short-lived and of no apparent significance to the question at hand. In the postintervention period Prime Minister Smith purged his party of opponents to majority rule and conducted an election to demonstrate his mandate to arrive at a settlement. Evidence of public relations activities after the contract date included frequent meetings between Smith and various western diplomats with maximum publicity, a guided tour for journalists to a village that had just suffered a guerrilla attack, attention to the "kidnapping" of children by Botswana-based guerrillas and to guerrilla attacks on Roman Catholic missions, an easing of the government's own conflict with the Roman Catholic church, a series of government reforms to enhance the apparent power of blacks, establishment of "protected villages" for blacks, establishment of a Rhodesian information office in Washington, and a report that white women throughout the country were contemplating suicide at the very thought of majority rule. Six months after the intervention, an article in the *New York Times* reviewed the government's public relations campaign, pointing out that it was effective in keeping the guerrillas' version of the war out of the press and in equating the black liberation movement with a communist insurrection (Windrich, 1977).

Mexico. The analysis period began in January 1975. By December of that year, Mexico was in many ways a classic example of a country with an image problem in the U.S. press. News coverage and commentary were focused on guerrilla attacks and subversion, repressive measures taken by the government, economic instability and dependence, political infighting, policy failures (particularly in land reform), tourist boycotts, and a general feeling of anti-Americanism. But unlike the other governments under review here, that of Mexico opted not to retain a public relations firm. The government did demonstrate concern over its image, particularly in an advertising campaign expressly intended to convince U.S. tourists and businesses that the country was not in fact anti-American, a tactic that runs directly contrary to the appropriate strategy indicated in Figure 3 for a country in Mexico's situation, but as a matter of choice it did not undertake a professionally orchestrated campaign of image polishing. During the year following this decision, coverage of social, political, and economic problems continued to dominate the news of Mexico. In fact, such news proliferated.

Because of the inherently selective and speculative nature of such evidence, however, and because some of these events undoubtedly took place for other, less image-related reasons as well, we do not find such anecdotal arguments in themselves to be meaningful indicators of public relations effects. Rather, we find them suggestive of behavioral tendencies on the part of both national actors and journalists which we believe to be associated with the public relations intervention. Only in combination with quantitative support for the impressions such data create can we accept with confidence their implications for the manipulation of information about, and ultimately images of, foreign nations in the United States press. It is in pursuit of such supporting evidence that we have undertaken the more systematic analysis of the effects of these various public relations efforts to which we now turn.

Quantitative Effects of the Public Relations Intervention

The most appropriate design for testing the effects of such interventions is what Campbell (1969), Cook and Campbell (1979) and others describe as an interrupted time series design. The question is whether a government policy or intervention (in this case, the public relations contract) changes the level or direction of a series of observations of theoretical or practical interest (such as coverage in the *New York Times*) subsequent to the intervention in sufficient degree to falsify the null hypothesis that such changes arise from chance. Where the magnitude of change makes chance factors a highly unlikely explanation, the analyst infers that it is attributable instead to the policy of intervention.

In applying an interrupted time series design to the present analysis, we regress a particular series of indicators for a given country (e.g., total references, negative references) on a binomially coded variable (0 or 1) indicating the absence or presence of effects of the intervention, then examine the result. The regression coefficient and its sign represent the size and direction of change in the dependent variable (a particular aspect of news coverage) attributable to the intervention. A t-test allows inferences about whether or not this change is substantially different from zero.[7] The coefficient of determination (r^2) indicates the explanatory power of the intervention for determining the entire series. Finally, a Durbin-Watson test for autocorrelation of residuals indicates whether the estimates are biased by serial correlation.

This latter test is important in this case because there are special problems inherent in time series analysis. Many time series are characterized by trends associated with growth or development over time. These and other autoregressive components that determine the series violate several assumptions of regression analysis, particularly one that requires that residuals must be substantially uncorrelated. Here residuals may be highly correlated unless certain trends and autoregressive components are removed. Positive serial correlation inflates the t-test so that a researcher might reject the null hypothesis when it is in fact true. This could become a problem for the present analysis if, for example, a series of events before contract was signed produced month-to-month increases in reporting. These effects can be estimated and controlled by a variety of methods. The most precise of these requires considerably more time points than we have included in our analysis. On the other hand, longer time periods would maximize noise and influence from extraneous sources. For relatively short series such as these, the most satisfactory method is application of a form of generalized least squares (GLS), the Cochrane-Orcutt iterative process. Essentially, this technique estimates the amount of damage from serial correlation and controls for its effects (Johnson, 1972). For purposes of the analysis, this technique will be applied whenever ordinary least squares (OLS) proves vulnerable to bias from autoregressive components of the series.

As in any quasi-experimental design, analysis of this type is vulnerable to plausible rival hypotheses as explanations of the observed effect. The effects of such reasonable alternatives should be handled either by logically ruling out their plausibility or by controlling for their effects. In this analysis, the independent variables for each country (the signing of the contract) are independent of one another. There is little logic or evidence that connects the signing of a public relations contract by the Republic of Korea, for example, with similar action by any other nation, apart from a general trend toward greater awareness of the importance of national images among many nations of the world. We noted no evidence of any comprehensive historical event that systematically affected reporting on all of the countries at the disparate times when their respective contracts were signed. Moreover, given that our analysis is based on several countries of differing size, location, and circumstance, the likelihood that localized, idiosyncratic random effects rather than the respective interventions account for our observations seems quite low. And the likelihood that five of the six governments in question hired a public relations consultant precisely three months before their images changed (and the sixth at the very moment of image change), and that the subsequent image change of each country was consistently in the direction predicted by our conceptualization irrespective of those public relations contracts, seems equally low.

Three separate strategies would seem to offer potential controls for the experimental effect. These include comparing coverage in the *New York Times* with corresponding coverage of the same nations and time periods in non-U.S. newspapers, comparing the experimental series against a random selection of series for various countries and time periods, or matching the experimental series with others that differ only with respect to the intervention itself. Let us examine each of these alternatives in turn.

The comparison of *New York Times* coverage of these nations with that in the foreign press would provide a check on the validity of the present research if and only if the effects of the public relations efforts by American firms are reasonably expected to be demonstrated only in U.S. media. This would be the case, for example, if the public relations counselors limited their activities to such visible devices as issuing press kits and press releases to U.S. news outlets. In reality, however, public relations activities are both more subtle and more pervasive, for they include the provision of training and advice that can change more fundamentally the policy behaviors of government, the packaging of those behaviors, or at the very least, the actions of a government to facilitate or to restrict the flow of information generally. Indeed, our conceptual framework suggests quite clearly that a campaign that defined public relations as simply the generating of more publicity would be counterproductive in many circumstances. Thus the immunity of the foreign press to these efforts cannot be safely assumed; its degree is an empirical question, and for that reason, reports in the foreign press cannot provide an unambiguous control for the present analysis. Their correspondence (or lack of correspondence) with U.S. news coverage represents less a test of the validity of our findings than a potential corollary hypothesis.

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[7] The t-test, usually an inferential statistic, is used here in a somewhat unorthodox manner. Some scholars argue that since a time series includes a series of observations, no inferential statistic is appropriate. We hold that the t-test can be used as a descriptive statistic to indicate significant fluctuations about a trend. Essentially, it measures whether the posttest series changes by an amount in excess of two standard errors (if the statistical criterion is .95). By definition, then, a change in excess of two standard errors is one that is significantly different from zero.

At first glance, the comparison of the experimental series against a random selection of series capturing coverage of various nations in various time periods appears more promising, but this promise is deceptive because once time series dependencies have been eliminated through procedures like those used here to initialize the data, every such series is stochastically generated in principle except for the intervention. Thus, even if we took one of our experimental series and started it at a different time (e.g., three months before the intervention rather than 12), it, too, might well appear to be stochastic. The likelihood of randomly selecting as a control a series where stochastic processes generate a pattern mimicking the experimental effect at the matching timepoint, which is to say, the likelihood of encountering a serious challenge to the validity of our findings, is extremely low. Accordingly, although such an approach would undoubtedly establish the apparent validity of our findings, we believe this result would be spurious.

A more genuine test of validity in the present circumstance is to identify a set of control series based on coverage of countries that are matched as closely as possible to the experimental cases, but for which an appropriate time period for comparison can be specified. In effect, what one needs here is a set of countries that at some point in time made a specific decision *not* to contract with an American public relations firm. Such a decision would constitute, in essence, a nonexperimental, time-specific event and would allow the analyst to identify a particular time series for comparison. This is by far the most rigorous test of validity for the present work, and it is the most appropriate, but because it requires the documentation of a specific nonevent at a specific time, its conditions are very difficult to satisfy. Nevertheless, it is this third approach to assessing validity that we have adopted here.

In actuality, we have been able to document only one appropriate control series, that for Mexico. As we have suggested above, events in Mexico and news coverage of them during the first half of the series resembled in key respects those in the experimental countries. In these regards, Mexico constitutes an acceptable match to a general profile of contracting countries. Where Mexico differs, again as noted earlier, is in its decision not to hire an American public relations counselor. This decision, taken in December 1975, defines the appropriate time series for comparison with the experimental cases and permits a genuine test of the experimental effect, which is to say, the impact of the public relations intervention. Although confidence in our results would surely be strengthened by the inclusion of more control series, we regard the identification of even one such case as most fortuitous given the evident constraints. This limitation notwithstanding, however, a comparison of this type is the most meaningful and least ambiguous test of validity available to us. And to the extent that the results of the Mexican nonexperiment differ from those of our other cases, our confidence in the validity of the observed effects of the public relations contracts is enhanced.

In the analysis that follows, regression parameters are estimated with a variety of assumptions about lag effects of the public relations contract. A guiding assumption is that changes in the time series might not be observed in the month in which the contract was signed, although in many instances virtually immediate consequences are detectable in the indicators. By minimizing the sum of squared residuals over all possible lags, however, we have found the best-fit equations to suggest a lag of three months before the impact of the intervention typically reaches its zenith. This compares with a typical lag variously estimated at six weeks to six months between changes in the media agenda and corresponding changes in the public agenda (Eyal, 1980; Stone & McCombs, 1981), and a similar lag of roughly one year between changes in the public agenda and corresponding changes in the policy agenda (Page & Shapiro, 1983). In the present instance, the observed lag conforms to what might reasonably be expected to constitute a start-up period for the public relations effort. Consequently, except where noted, the equations reported below are estimated with a three-month lag assumption, the full force of the intervention beginning in the fourth month.

All of the nations selected for the present study had negative pre-intervention images, but the degrees and components of those images did differ. Table 1 presents seven indicators that suggest the image problems faced by these countries with regard to their coverage in the U.S. press. Two of these are especially noteworthy.

The first, percent negative reporting, represents the proportion of all articles about a country that can be characterized as negative. According to this indicator, Argentina clearly had the most negative press image. Fairly close behind came the Philippines, Republic of Korea, Mexico, Rhodesia, and Indonesia. In contrast, Yugoslavia had a much less negative image in the press, with about 24% negative references. But this ranking on negative image, which might be considered a major target of public relations efforts, can be misleading if viewed in isolation. For as our earlier discussion made clear, a high proportion of negative references may well be partially offset by low scores on a second factor, visibility. Thus Rhodesia, which ranks only fifth among the countries under review in its proportion of negative mentions, but which received roughly two to three times the press attention of any other country and had the greatest absolute imbalance of negative over positive references (over 17 per month in the pre-intervention period), probably faced the most difficult public relations challenge of all the countries under review.

Table 2 indicates the changes in image associated with a public relations contract related to each country's initial position on summary indicators of visibility and valence. As the table shows, the Republic of Korea and Argentina, which ranked initially among the top three countries in visibility and possessed the two most negatively valenced images (Table 1), each experienced decreases in visibility of approximately 11 articles per month and a corresponding doubling of the positive valence, both associated in time with the signing of the public relations contract and both wholly consistent with our expectations. The Philippines and Yugoslavia, whose relatively low initial visibility would seem to position them especially well to make positive changes in the valence of their respective images, show a slightly different pattern. Here movement centered on a reduction in visibility; valence was essentially unchanged. This observation may reflect either a partially successful effort at manipulation along both dimensions, the employment of an alternative unidimensional strategy designed to curtail negative reporting by the simple expedient of reducing all reporting, or the operation of a regression effect in which the low initial visibility of these countries means that even with substantial proportionate reductions on this indicator it is very difficult to achieve correspondingly significant increases in positive valence. Whichever explanation holds, the net effect of the public relations contract in both cases was an improvement in image centered on changes in visibility.

Despite substantial movement in the predicted direction, the data on Rhodesia show no statistically significant changes in either of the indicators. In general both visibility and valence in this case appear to be functions of reporting on violence in the guerrilla war between the Smith regime and insurgent black nationalists. As we have shown elsewhere (Albritton & Manheim, 1983), significant reductions in reporting of violence are associated with the public relations contract. In fact, a detailed examination of this reporting adds considerable support to the notion that news coverage is responsive to public relations activities and even offers some insights into the differential effects that such public relations campaigns may have on particular classes of information and reporting.

Indonesia's very low visibility before it signed a public relations contract permits us to test our expectation that such conditions provide an opportunity to effect important changes in the valence component of national image. Table 2 demonstrates significant movement in precisely the direction we had anticipated. One very important difference between this case and the others is that there is no significant change in visibility associated with the public relations contract. Instead, there is a significant positive shift in valence of approximately 14%. Moreover, again in contrast with the other contracting nations, this positive shift in the valence component is not a result of reduced negative reporting. To the contrary, the only other significant change associated with the intervention (not shown) is a more than doubling in the frequency of positive reporting. Thus in the one contracting nation where conditions posited by our conceptualization for making significant positive changes in the valence component of image are especially favorable, we find the only instance in which significant increases in positive reporting (rather than reduction of negative mentions) are associated with introduction of the public relations contract, and the only instance in which positive image change is centered wholly on the valence dimension.

In this light, Mexico offers an especially interesting contrast. At the time it rejected a contract, Mexico was in one of the most advantageous positions of any nation we examined. Its visibility was even lower than Indonesia's, providing an opportunity to duplicate that nation's success, and its valence (44.5% positive) was ex-

Table 1. Average Monthly Scores of Seven Nations on Selected Indicators of Image Visibility and Valence during the Pre-Contract Period

	Republic of Korea	Argentina	Philippines	Yugoslavia	Rhodesia	Indonesia	Mexico
Total articles (*N*)	18.3	21.6	10.3	10.2	54.3	5.1	3.7
Articles with negative valence (*N*)	9.8	16.1	7.2	2.6	26.6	1.3	1.9
Articles with positive valence (*N*)	2.8	1.9	2.7	1.7	9.2	0.7	1.1
Articles with negative valence as % of total	56.1	75.0	68.2	23.7	48.5	46.4	49.5
Articles with positive valence as % of total	14.8	8.0	30.2	17.9	23.0	33.1	38.3
% positive of all valenced articles	23.0	10.0	29.9	49.8	30.7	34.2	44.5
Net of positive-negative articles (*N*)	−7.0	−14.1	−4.5	−0.8	−17.4	−0.6	−0.7

Table 2. Estimation of Change in Visibility and Valence Associated with a Public Relations Contract

	Republic of Korea	Argentina	Philippines	Yugoslavia[a]	Rhodesia	Indonesia	Mexico[b]
Visibility (total number of articles)							
Equation (type of estimation)	OLS	OLS	GLS	OLS	GLS	OLS	OLS
Coefficient of regression (b)	−11.37	−10.71	−8.92	−5.17	−17.61	0.09	6.11
t-test	3.93[c]	3.95[c]	3.22[c]	2.67[c]	1.19	0.03	3.92[c]
Coefficient of determination (r^2)	.40	.42	.71	.25	.20	.00	.41
Durbin-Watson coefficient	1.38	2.16	1.88	2.23	2.05	1.39	1.66
Postcontract mean	7.6	10.9	4.0	5.1	43.2	5.2	9.8
Valence (percent positive of all valenced articles)							
Equation (type of estimation)	OLS	OLS	OLS	OLS	OLS	GLS	OLS
Coefficient of regression (b)	23.80	11.76	1.22	−3.33	3.96	12.95	−3.87
t-test	2.75[c]	2.78[c]	0.11	0.23	0.62	2.31[c]	0.29
Coefficient of determination (r^2)	.25	.26	.00	.00	.02	.52	.00
Durbin-Watson coefficient	1.91	1.59	2.24	1.96	1.40	2.21	1.63
Postcontract mean	54.6	22.0	29.6	46.4	32.6	46.7	40.7

[a] Estimated with no lag.

[b] Estimated with no lag. However, similar estimates with a three-month lag showed no significant differences from the results reported here.

[c] Change exceeds two standard errors.

ceeded only slightly by Yugoslavia's and, although still generally negative, was substantially less so than that of any other nation considered here. Yet the pattern of image development following rejection of a contract is strikingly different from our other observations. Not only do the data reflect an absence of progress in enhancing positive valence, but there is actually a decline to 40.7% positive mentions. In addition, visibility increases dramatically, more than doubling during the postrejection period. The net result is virtually the opposite of that found in nations operating with public relations assistance. Instead of using its initially favorable position to enhance its valence, Mexico appears to move in precisely the wrong direction, gaining visibility while its valence slips.

To reconcile our observations regarding the public relations efforts undertaken (or not undertaken) in behalf of these seven nations with the analytical framework that we developed earlier, we have imposed specific parameters on the graphic representation of dimensions of national image first presented in Figure 3 and have located each of our countries in the appropriate positions for the pre- and postintervention periods on the two indicators reported in Table 2.[8] The result is illustrated in Figure 4.

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[8] We have arbitrarily set the maximum value of visibility at 60 articles per month for purposes of illustration. Our theory deals with the relative positions of national images more than with absolute distances, and the most salient point here is that Rhodesia's image is clearly and substantially more visible than that of any other nation under review.

Figure 4. Directions of Image Change

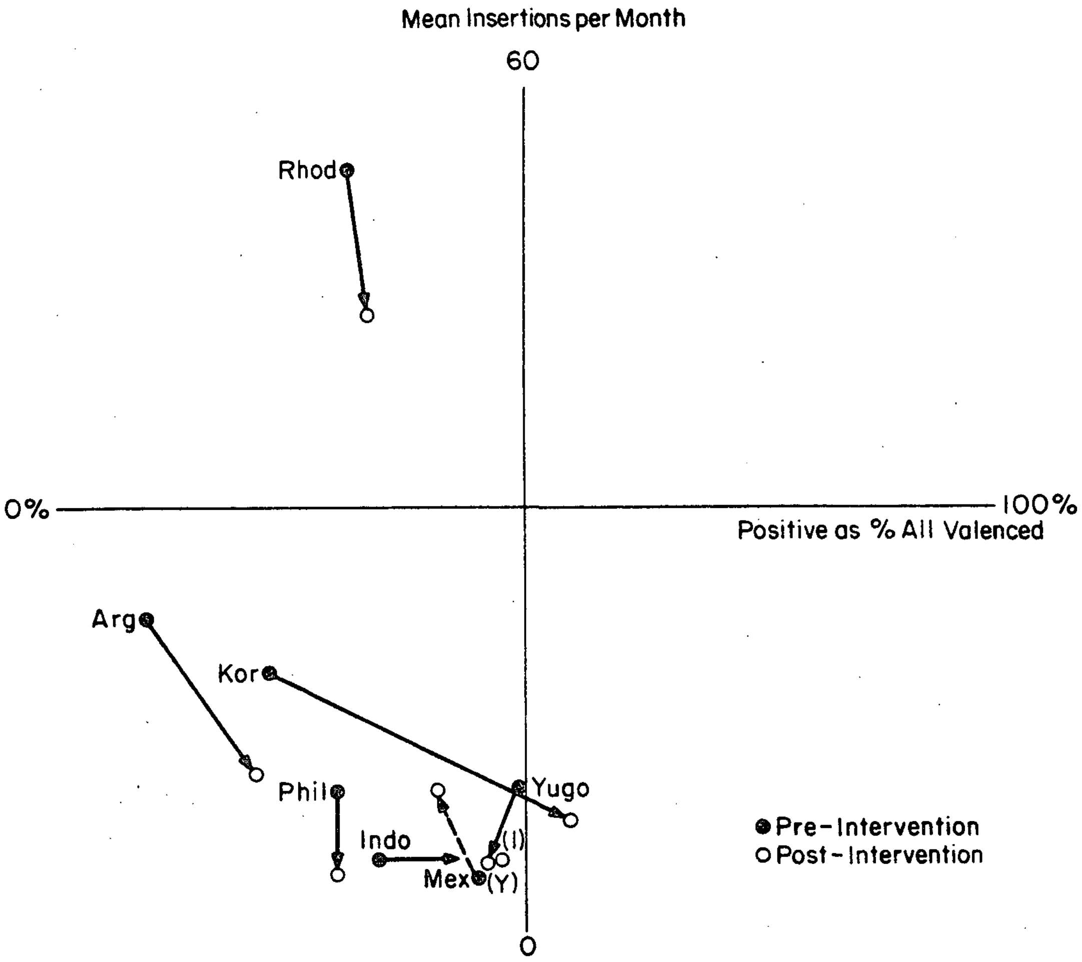

Table 3. A Comparison of Goodness of Fit Criteria to Zero-Order Polynomial and Parabola Specifications for Changes in Visibility and Valence of National Images as a Result of Contractual Relationship with American Public Relations Firms (N=6, Mexico Excluded)

	Zero-Order Polynomial	Parabola
Visibility		
R^2	.9726	.9970
Sum of squared residuals	31.55	3.39
Valence		
R^2	.3005	.3841
Sum of squared residuals	542.08	477.33

Within this framework, we can see that Rhodesia begins as rather clearly a quadrant 1 country, whereas the others are initially distributed at various points within quadrant 2; that is, Rhodesia scores high on visibility and low on positive valence, whereas the others score low on both visibility and positive valence. A glance at the data in Table 1 and a review of the above discussion should make clear not only that these categorizations are correct, but that the categories themselves are substantively meaningful.

The strategy of influence specified in Figure 3 suggests that, as a result of contracting with an American public relations firm, a nation's image will shift on both visibility and valence in the form of a parabolic arc. A key question is whether the empirical data summarized in Figure 4 fit this specification. The fact that both the pre- and postcontract locations represent coordinates rather than unidimensional values poses problems for conventional methods of curve-fitting. Still, the theory stipulates that when a contract is in effect, the shift from the precontract to the postcontract locations is a function of both coordinates in the precontract period. One way to test the theory under this specification is to determine how well visibility and valence in the postcontract period separately can be predicted from the values on both dimensions before the contract is signed. Since we are here assessing only how well the observations fit the theory, the appropriate tests are simply those that measure goodness of fit, i.e., the correlation coefficient and the distribution of residuals.

Table 3 shows a strong relationship between the theory and the data. The visibility dimension in the postcontract period is almost wholly determined by the model. The inference is that movement on this dimension associated with the public relations contract is uniformly in the expected direction. Movement on the valence dimension is not as strongly associated with the contract, but the theory does account for some 38% of the variance on this dimension as well.

Perhaps the most important implications of the data reported in Table 3 derive from the comparison of goodness-of-fit criteria for zero-order polynomial and parabolic arc specifications respectively. Although changes in the R^2 indicate only a moderate relative advantage for the parabolic model, examination of the sums of squared residuals shows that a parabolic specification represents a significant improvement over at least one important alternative in fitting the data. Thus the movement of the data from the precontract to the postcontract period in the form of a parabola, although inconclusive because of the small number of cases, does offer substantial support for the theoretical specification in Figure 3.

Discussion

Taken together, these results suggest two conclusions. First, there exists a clear temporal correspondence between the signing of a public relations contract by a given nation and shifts in the image of that nation as portrayed in the *New York Times*. We have observed this correspondence across six nations of varying size, location, and circumstance, and found it absent only in a seventh, noncontracting nation whose pretest image was comparable to those of the others. Second, those image changes that did occur in association with the public relations contracts were consistent with the pattern suggested by the literature we have cited and with the predictions of our own conceptual framework. Put another way, our findings are precisely those we would expect if the public relations consultants in question were pursuing a set of strategies predicated on the same arguments we have raised, and if they were doing so effectively.

That such persuasive efforts might take this form should come as no surprise. The literature on attitute stability and change has long recognized the importance to persuasion of controlling the flow of information and the degree of discrepancy between new messages and existing cognitions (summarized in Kiesler, Collins, & Miller, 1969); that on the diffusion of innovation has almost literally been built on the role of awareness in the movement of behavior-related information (summarized in Savage, 1981); and that on propaganda, although using rather different terminology, has applied many of the same lessons (summarized in Kecskemeti, 1973). Wolfsfeld's (1983) recent work, with its synthesis of the attitude change and propaganda literatures, provides a conceptual bridge between microlevel psychological processes and macrolevel communication strategies that makes clear the interdependence of image content, awareness, and persuasibility.

What is surprising, however, is the lack of recognition of the first and more fundamental point, that external actors not only try systematically to influence the media-public-policy agenda system, but that they succeed. For our data clearly demonstrate that public relations efforts in behalf of foreign countries do give rise to changes in the media portrayal of those countries. In case after case, significant improvements in projected images follow upon the signing of the public relations contract. Clearly, then, efforts to analyze the *interactions that occur within* the agenda system must take into account external environmental factors that may well influence such exchanges.

We do not contend that (projected) image improvements of the type we have observed translate immediately into public (perceived) image or public policy bonanzas for the various client states, or even that they will be associated with particular, identifiable behaviors on the part of either voters, members of issue publics, or policymakers. To the contrary, we have specifically distinguished between these efforts at persuasive communication and others, such as political campaigns, that are directed at inducing particular behaviors. What these more subtle activities may well accomplish, however, as Merritt (1980) recognized, is a change in the general informational context within which a country is perceived, and a secular shift in the propensity of either mass or elite political actors to behave supportively toward it when such behavioral opportunities present themselves. (For an example, see Smith, 1970.) Given the flow of information and the behavioral connections among the three agendas that are becoming increasingly evident in the literature on agenda setting, these propensities may well translate over time into policy outputs of real value to the countries in question.

The literature on agenda setting has established a series of linkages that tie projected images in the media to perceived images among the public, and, in turn, perceived images among the public to the policy agenda and its products. Although various details of these interconnections remain to be documented, the notion of linkage and the workings of its basic mechanisms are widely recognized. The role of mass media in generating and disseminating images of reality that channel political debate assumes special importance in this conceptualization. The media are seen as gathering information from the public, policymakers, and the external environment, then packaging that information for consumption. The product of this activity becomes a primary source of both public opinion and policy action. The present research suggests that actors external to this process are not necessarily passive or benign, that to the extent of their dependence on such actors as sources of information the media are vulnerable to manipulation, and, by extension, that such manipulation of projected images of reality has the potential to influence public opinion and the policy process.

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Fear and Trembling: Terrorism in Three Religious Traditions

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As the first comparative study of religious terror groups, the article provides detailed analyses of the different doctrines and methods of the three best-known groups: the Thugs, Assassins, and Zealots-Sicarii. Despite a primitive technology, each developed much more durable and destructive organizations than has any modern secular group.

The differences among the groups reflect the distinguishing characteristics of their respective originating religious communities: Hinduism, Islam, and Judaism. The distinctive characteristics of religious terror are discussed, and relationships between religious and secular forms of terror are suggested.

In 1933 *The Encyclopaedia of the Social Sciences* published fascinating, useful articles on assassination (Lerner) and terrorism (Hardman), which ended on a strange note, namely that the phenomena, which had reached an exceptionally high point at the turn of the century, were declining so much that the subjects would remain interesting only to antiquarians. Future events would be determined by classes and masses, because modern technology had made our world so complex that we had become increasingly *invulnerable* to determined actions by individuals or small groups. Terrorist activity became extensive again after World War II, not in Europe and America, as was the case earlier, but in western colonial territories, particularly in the Palestine Mandate, Cyprus, Malaya, Kenya, Vietnam, and Algeria. But the second edition of the *Encyclopedia,* which was published in 1968, ignored both subjects; perhaps the editors believed the prophecies in the earlier edition!

Academics returned to the subject when terrorist activity revived again in the center of the western world. The flow of articles and books began in the 1970s, and that flow continues to increase every year. A journal entitled *Terrorism* has been established, and many universities offer courses on the subject. As they did 50 years ago, political scientists dominate the field, and in some respects the conventional wisdom governing terrorist studies has not changed: the technological, not the political, environment is normally seen as the decisive determining condition for terrorist activity. Many contemporary studies begin, for example, by stating that although terrorism has always been a feature of social existence, it became "significant" for the first time in the 1960s when it "increased in frequency" and took on "novel dimensions" as an international or transnational activity, creating in the process a new "mode of conflict."[1] The most common explanation for this "new mode of conflict" is that now we are experiencing the cumulative impacts of specific develop-

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[1] "Terrorism is an activity that has probably characterized modern civilization from its inception. In the past decade, however, terrorist activity has increased in frequency and has taken on novel dimensions. For example, incidents are being employed more as a means of political expression and are becoming characterized by a transnational element" (Sandler, Tshirhart, & Cauley, 1983, p. 36). The phrase "new mode of conflict" was coined by Jenkins (1975). See also Mickolus (1980, Introduction) and Hacker (1976, Preface). As is often the case with conventional wisdom, the view is expressed without elaboration in the first paragraph or preface. To Gurr (1979, p. 23), the "conventional wisdom (concerning terrorism) is a fantasy accepted as an ominous political reality by (virtually) everyone." *Cf.* Rapoport (1982a, Introduction).

ments in modern technology. Individuals and tiny groups have capacities that they previously lacked. Weapons are cheaper, more destructive, easier to obtain and to conceal. "The technological quantum jumps from the arrow to the revolver and from the gun to the Molotov Cocktail" (Hacker, 1976, p. ix). Modern communications and transport allow hitherto insignificant persons to coordinate activity quickly over vast spaces. Finally, by giving unusual events extensive coverage, the mass media complete the picture. "You can't be a revolutionary without a color TV: it's as necessary as a gun" (Rubin, 1970, p. 108).

It is in the nature of conventional wisdom that we never feel obliged either to examine or to demonstrate its propositions. The historical illustrations we provide are decorative, and we analyze contemporary experiences as though the statement declaring them *suis generis* is itself clear and at the same time provides the only evidence needed to establish the case! Although one can never be sure of what is meant by the term "modern terrorism," the characterizations normally focus on increases in the number of incidents or amounts of damage and on the fact that assaults transcend state borders. Because early experiences are insignificant in these respects, they are deemed irrelevant. One purpose of this article is to show that this view is simply wrong and that the past can provide materials for useful comparisons.

I shall do this by a detailed analysis of three groups: the Thugs, the Assassins, and the Zealots-Sicarii.[2] I have chosen them for several reasons. They are the examples most often cited to illustrate the ancient lineage of terrorism, but they are not discussed in our literature. We cite them because they are so well known elsewhere; no other early terror group has received as much attention. Ironically, although the words thug, assassin, and zealot have even become part of our vocabulary (often to describe terrorists), and most educated persons can identify the groups, they have never been compared.[3]

The cases are inherently interesting and peculiarly instructive. Each group was much more durable and much more destructive than any modern one has been; operating on an international stage, they had great social effects too. Yet the noose, the dagger, and the sword were the principal weapons they employed, travel was by horse or foot, and the most effective means of communication was by word of mouth. Although a relatively simple and common technology prevailed, each example displayed strikingly different characteristics. The critical variable, therefore, cannot be technology: rather, the purpose and organization of particular groups and the vulnerabilities of particular societies to them are decisive factors. Although the point may be more easily seen in these cases, it must be relevant, I shall argue, in our world too.

Furthermore, the three cases illustrate a kind of terror nowhere adequately analyzed in our theoretical literature, terror designated here as holy or sacred (cf. Laqueur, 1977; Price, 1977; Rapoport, 1971, 1977, 1982a; Thornton, 1964; Walter, 1969). Before the nineteenth century, religion provided the only acceptable justifications for terror, and the differences between sacred and modern expressions (differences of nature, not scale) raise questions about the appropriateness of contemporary definitions. The holy terrorist believes that only a transcendent purpose which fulfills the meaning of the universe can justify terror, and that the deity reveals at some early moment in time both the end and means and may even participate in the process as well. We see terrorists as free to seek different political ends in this world by whatever means of terror they consider most appropriate. This trait characterizes modern terrorism since its inception in the activities of Russian anarchists more than a century ago, and it is found also in many modern terrorist organizations in our century which have had important religious dimensions, i.e., the IRA, EOKA (Cyprus), the FLN (Algeria), and the Irgun (Israel). Sacred terror, on the other hand, never disappeared altogether, and there are signs that it is reviving in new and unusual forms.

As instances of sacred terror, the Thugs, the Assassins, and the Zealots-Sicarii seem remarkably different from each other, and hence they provide some orientation to the range of possibilities associated with the concept. On the other hand, each closely resembles other deviant groups within the same parent religion, Hinduism, Islam, and Judaism, and the three kinds of deviant groups reflect or distort themes distinctive to their particular major religion.[4] In the last respect, what seems to be distinctive about modern terrorists, their belief that terror can be organized rationally, represents or distorts a major theme peculiar to our own culture: a disposition to believe that any activity can be made rational.

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[2] I do not distinguish Zealots from Sicarii, although they are distinctly different groups, as Smith (1971) demonstrates. The Sicarii terrorized mostly Jews, whereas the Zealots were more concerned with Romans and Greeks. But for our purposes this is not a critical distinction. A more extensive discussion of the Jewish uprising appears in Rapoport (1982b). Horsley (1979a) is the only other essay I know which discusses the Jewish activity as terrorist activity.

[3] The cases are so well known and interesting that Thomas DeQuincey (1877), a nineteenth-century Romantic writer and the first student of comparative terrorism, pointed out the importance of comparing them. DeQuincey himself concentrates on the *Sicarii* in various essays. Lewis (1967, chap. 6) compares the three briefly.

I shall begin with a detailed analysis of the cases and in an extended conclusion draw out some implications and comparisons. My concern is largely with methods and doctrines, not the social basis of group activity. The order of the presentation (Thugs, Assassins, and Zealots-Sicarii) is designed to carry the reader from situations where only religious ends are served to one where the political purpose seems, but in fact is not, altogether dominant. The order also illustrates an irony, namely that there can be an inverse relationship between proximity in time and distance from us in spirit. Although extinguished in the nineteenth century, the Thugs seem wholly bizarre because they lacked a political purpose, and we invariably treat terror as though it could only serve one. The Assassins, who gave up terror in the thirteenth century, are comprehensible because their ends and methods remind us of nineteenth-century anarchists who originated modern rebel terror and were themselves conscious of affinities. But it is the Zealots-Sicarii, destroyed in the first century, who appear almost as our true contemporaries because they seem to have purposes and methods that we can fully understand. By means of provocation they were successful in generating a mass insurrection, an aim of most modern terrorists, but one that has probably never been achieved. The purpose of the Zealots-Sicarii, it seems, was to secure national liberation inter alia. The striking resemblances between their activities and those of terrorists with whom we are familiar will put us in a better position to conclude by elaborating the differences already suggested between holy and modern terror.

Thugs

"Terror," Kropotkin wrote, is "propaganda by the deed." We are inclined to think of it as a crime for the sake of publicity. When a bomb explodes, people take notice; the event attracts more attention than a thousand speeches or pictures. If the terror is sustained, more and more people will become interested, wondering why the atrocities occurred and whether the cause seems plausible. Hence virtually all modern conceptions of terrorism assume that the perpetrators only mean to harm their victims incidentally. The principal object is the public, whose consciousness will be aroused by the outrage.

For the holy terrorist, the primary audience is the deity, and depending upon his particular religious conception, it is even conceivable that he does not need or want to have the public witness his deed. The Thugs are our most interesting and instructive case in this respect. They intend their victims to experience terror and to express it visibly for the pleasure of Kali, the Hindu goddess of terror and destruction. Thugs strove to avoid publicity, and although fear of Thugs was widespread, that was the unintended result of their acts. Having no cause that they wanted others to appreciate, they did things that seem incongruous with our conception of how "good" terrorists should behave.

Indeed, one may ask, were the Thugs really terrorists? They are normally identified as such in the academic literature (DeQuincey, 1877; Freedman, 1982; Gupta, 1959; Laqueur, 1977; Lewis, 1967). As persons consciously committing atrocities, acts that go beyond the accepted norms and immunities that regulate violence, they were, according to one established definition, clearly terrorists.[5] Their deceit, unusual weapon (a noose), and practice of dismembering corpses (thereby preventing cremation or proper burial) made Thug violence outrageous by Hindu standards, or, for that matter, by those of any other culture. Cults of this sort may not exist anymore, but as the case of the Zebra Killers or the Fruit of Islam in San Francisco in 1975 demonstrates, the religious purposes of a group may prescribe murders that the public is not meant to notice.[6] A city was terrorized for months, but no one claimed responsibility. It is doubtful whether any American terrorist group produced as much panic as this one did, although terror may not have been its purpose.[7]

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[4] It would be useful to extend the analysis by treating Christian terror, but the materials are not as conveniently available. No single Christian terror group has caught the public imagination in a way that is comparable to those I have chosen. Unlike those groups discussed here, the numerous millenarian sects using terror in the late medieval period did not rely on hit-and-disappear tactics. Their terror was a sort of state terror; the sects organized their communities openly, taking full control of a territory, instituting gruesome purges to obliterate all traces of the old order, and organizing large armies, which waged holy wars periodically sweeping over the countryside and devastating, burning, and massacring everything and everyone in their paths. The military pattern reminds one of the Crusades, an unlimited or total war launched by the Papacy (Cohn, 1961; cf. Rapoport & Alexander, 1982), in which seven essays discuss relationships between sacred and modern justifications, focusing largely on Christian traditions.

[5] Although the Thugs may do what they do because they know that ordinary Hindus regard such actions as terrifying and horrible, they want victims only to experience terror. The earliest contemporary discussions of terrorism emphasized the extranormal character of its violence as the distinguishing feature, but the importance of that distinction has been largely lost. Compare Thornton (1964), Walter (1969), Rapoport (1977), and Price (1977). Since terror is extranormal violence, it is likely to flow initially from a doctrine, and it tends to be a historical rather than a universal phenomenon. In recent years our definitions generally treat terror and violence as synonyms. (See, for example, Russell, 1979, p.

No one knows exactly when the Thugs (often called Phansigars or stranglers) first appeared. Few now believe that the ancient Sagartians, whom Herodotus (VII, 85) describes as stranglers serving in the Persian army, are the people whom the British encountered in India some 2500 years later.[8] But there is evidence that Thugs existed in the seventh century, and almost all scholars agree that they were vigorous in the thirteenth, which means that the group persisted for at least six hundred years.[9] By our standards, the durability of the Thugs is enormous; the IRA, now in its sixth decade, is by far the oldest modern terrorist group.

There are few estimates of the number of people killed by the Thugs. Sleeman (1933) offers a conservative figure of one million for the last three centuries of their history.[10] This figure seems

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4.) Since violence is a universal phenomenon, it is not surprising that there is a tendency for those who do not distinguish between violence and terror to treat differences in the latter as largely differences in scale. Hostile sources compiled the materials for all three groups, which poses important questions of reliability. Specific footnotes for each case treat these problems, although obviously only historians of each period can assess the documents adequately. The pictures drawn for each group differ so dramatically that at the very least they represent archetypes of specific religious traditions.

[6] When early twentieth-century Hindu terrorist groups used Kali to justify their activities, secrecy was shunned because they had a political purpose, the independence of India (Pal, 1932).

Because terror can give the perpetrator joy, it can be undertaken for its own sake. An example might be the Tylenol killer in the fall of 1982, who laced capsules with arsenic, terrorizing the American public and drug industry in the process. Publicity would be important in this case of terror for terror's sake only if the terrorist desired an audience too.

[7] The experience is described in a reasonably accurate, overly gruesome bestseller (Clark, 1979). The group apparently believed that a race war would develop from its efforts, and perhaps at this point it would become visible.

[8] Primary sources on the Thugs are extensive. Numerous archival and published government materials exist for virtually every year from 1826 to 1904, the latter being the termination date of the special Indian institution created to deal with Thuggee and related problems, The Thag and Dakaiti Department. By 1850 Thug activity itself ceased almost entirely. Pfirrmann (1970) is the only person who has examined all the primary source materials. His conclusions are substantially those offered by W. H. Sleeman, the remarkable officer who made the Thugs an issue in British politics, contrived the special methods used to destroy them, and proved to be a perceptive sociologist of religion. Sleeman's six published books (1836, 1839, 1840, 1893, 1858, and 1849) are listed in order of their pertinence. Two useful nineteenth-century secondary accounts based on Sleeman are Hutton (1857) and Thornton (1837). The best twentieth-century books published before Pfirrmann are Sleeman (1933) and Bruce (1968).

The Thugs have captured literary imaginations. Meadows Taylor, a British officer with Sleeman, wrote a bestselling novel (1839) which was reprinted several times. Wilkie Collins's novel, *The Moonstone,* has gone through eleven editions at least, and John Masters (1952) has provided the latest fictional account.

[9] The thirteenth-century writings of Jalalu-d din Firōz Khilji, Sultan of Delhi, refer to the banishment of a thousand persons generally identified as Thugs. But before their demise, not much was known about them. Afterward, the thoroughness of British officials, trial records, and police informants provided much material. Although the information was compiled by British police administrators and the Thugs were denied public trials, legal counsel, and the right to question witnesses, the picture developed from this information was accepted completely for more than a century. Recently, it was challenged by Gupta (1959) and Gordon (1969), who believe that the group developed only when the British arrived. Gupta provides no evidence for this view, and Pfirrmann is justified in simply brushing it aside as a polemic. Gordon's thesis seems more substantial and depends on allegations of inconsistencies in the primary sources. His essay was published too late for Pfirrmann to evaluate, but I found that the inconsistencies cited come largely from Gordon's tendency to take quotations out of context, which may explain why he did not develop the thesis in subsequent writings and why it has been ignored by others.

[10] The estimate is incorporated in J. L. Sleeman's title (1933). Every estimate flounders because we don't know the age of the organization or its size in various periods. It is generally assumed that the number remained constant because the group was largely hereditary. In my view, the administrative chaos that prevailed in the wake of the Moghul Empire's collapse when the British arrived gave the brotherhood unusual opportunities for new victims and swelled its ranks, which suggests that Sleeman's "conservative estimate" represents a maximum, not a minimum, one.

too large, but half that number may be warranted, and that, indeed, is an astonishing figure, especially when one remembers that during the life of modern terrorist organizations, the deaths they cause rarely exceed several hundred, and it would be difficult to find one group that is directly responsible for more than ten thousand deaths.[11] The Thugs murdered more than any known terrorist group, partly because they lasted so much longer. Their impact on Indian economic life must have been enormous, although there is no way to calculate it. If the significance of a terrorist group is to be understood by these measures, the Thugs should be reckoned the most important ever known. The paradox is that, unlike most terrorist groups, they did not or could not threaten society for the simple reason that their doctrine made them attack individuals rather than institutions.

The reinterpretation of a cardinal Hindu myth and theme provided the Thugs with their peculiar purpose and method. Orthodox Hindus believed that in early times a gigantic monster devoured humans as soon as they were created. Kali (also known as Bhavani, Devi, and Durga) killed the monster with her sword, but from each drop of its blood another demon sprang up, and as she killed each one, the spilled blood continued to generate new demons. The orthodox maintained that Kali solved the problem of the multiplying demons by licking the blood from their wounds. But the Thugs believed that Kali sought assistance by making two men from her sweat who were given handkerchiefs from her garment in order to strangle the demons, that is, kill them without shedding a drop of blood. Upon completing their mission, they were commanded to keep the handkerchiefs for their descendants.

In Hindu mythology Kali has many dimensions. She represents the energy of the universe, which means, as the legend suggests, that she both sustains and destroys life. She is also the goddess of time, who presides over endless cycles in which both essential aspects of the life process are carried out. The Thug understood that he was obliged to supply the blood that Kali, his creator, required to keep the world in equilibrium. His responsibility was to keep himself alive as long as possible so that he could keep killing, and it has been estimated that each Thug participated in three murders annually: one claimed to have helped strangle 931 persons.[12] No one retired until he was physically unable to participate in expeditions. The logic of the cycle or balance required the brotherhood to keep its numbers relatively constant. New recruits came largely from the children of Thugs, and the deficiencies were made up by outsiders. The children were initiated into the tradition early by a carefully calculated gradual process—a circumstance that contributed to their resoluteness. Adult Thugs never seemed to experience revulsion, but sometimes the young did; invariably the cases involved those who witnessed events before they were supposed to. Drugs were used rarely, and then only among the young.

For obscure religious reasons Thugs attacked only travellers, and although they confiscated the property of their victims, material gain was not their principal concern, as indicated by their custom of "distinguish(ing) their most important exploits" not by the property gained but "by the number who were killed, the Sixty Soul Affair . . . the Sacrifice of Forty" (Russell & Hira, 1916, vol. 4, p. 567). The legend of their origin also shows murder to be the Thugs' main business, murder in which the death agony was deliberately prolonged to give Kali ample time to enjoy the terror expressed by the victims. It was forbidden to take property without killing and burying its owner first. The Thugs judged the ordinary thief as morally unfit.[13] When religious omens were favorable, many without property were murdered. Similarly, unfavorable omens protected rich travellers.

Although murder was the Thugs' main object, they needed loot—enormous quantities of it—to pay princes who provided their expeditions with international sanctuaries. Without those sanctuaries the brotherhood would not have persisted for such a long time. As we have learned again and again in the contemporary world, when international sanctuaries are provided, relations between states are exacerbated constantly. After numerous frustrating experiences. British authorities decided that appropriate cooperation from neighboring native states was not forthcoming. Nor did recourse to doctrines of hot pursuit prove adequate (Sleeman, 1836, p. 48).[14] Ultimately, the international law governing piracy was utilized, enabling British officials to seize and punish Thugs wherever they were found. The cost was a more massive violation of the rights of independent states, culminating in a direct expansion of imperial jurisdictions, the result that critics of the policy feared most.

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[11] When terrorist activities are part of a larger military struggle (i.e., Vietnam and Algeria), we have no reliable statistics on the terror alone. In situations when terror alone prevails (e.g., Cyprus, Aden, Northern Ireland) the casualties terrorists inflict rarely exceed three figures.

[12] "Bhowanee is happy and most so in proportion to the blood that is shed. . . . Blood is her food. . . . She thirsts for blood!" (Sleeman, 1836, p. 36). The estimates made by various British officials are compiled in a review article which also provides a list of 20 leading Thugs who murdered 5120 persons, an average of 256 each (A religion of murder, 1901)!

[13] "There are many thieves in my village but I would not go with them. My father Assa used to counsel me against the thieves saying—do not join them, they take money without thugging. Go with Thugs. If I had a (farthing) by Thuggee, I would take it, but never by theft" (Pfirrmann, 1970, p. 70). Another on-the-spot observer, Sir John Malcolm (1823, vol. 2, p. 187), suggested that robbery was the prime concern, "their victims . . . are always selected for having property. . . ." But the evidence seems to be clearly against him.

A striking feature of Thug operations was that virtually all activity was hemmed in by self-imposed restraints. From the moment he joined an annual sacred expedition until it was disbanded, a Thug was governed by innumerable rules, laid down by Kali, that specified victims, methods of attack, divisions of labor, disposal of corpses, distribution of booty, and training of new members. In a sense, there were no choices to be made because in dubious circumstances Kali manifested her views through omens.

British observers were impressed with the extraordinary "rationality" of the rules established. "Whatever the true source may be, (the system) is beyond all doubt the work of a man of genius, no ordinary man could have fenced and regulated it with so elaborate a code of rules—rules which the Thugs seem to believe are of divine origin, but in each of which we can trace a shrewd practical purpose" (Sleeman, 1839, p. 31).[15] "Ridiculous as their superstitions must appear . . . they serve the most important purposes of cementing the union of the gang, of kindling courage, and confidence; and by an appeal to religious texts deemed infallible of imparting to their atrocities the semblance of divine sanction" (A religion of murder, 1901, p. 512). "The precautions they take, the artifices they practice, the mode of destroying their victims, calculated at once to preclude any possibility of rescue or escape—of witnesses of the deed—of noises or cries for help—of effusion of blood and, in general of trades of murder. These circumstances conspire to throw a veil of darkness over their atrocities" (Sherwood, 1820, p. 263).

The list of persons immune from attack—women, vagabonds, lepers, the blind, the mutilated, and members of certain artisan crafts (all considered descendants of Kali, like the Thugs themselves)—suggests, perhaps, that the cult may once have had a political purpose. Nonetheless, there can be no politics without publicity.

Whatever purpose these rules were designed to serve, they could not be altered even when the life of the brotherhood was at stake, because they were perceived to be divine ordinances. Europeans, for example, were immune from attack—a prohibition that virtually enabled Thugs to escape attention. When the Thugs were discovered, the same rule kept them from retaliating directly against the small, relatively unprotected group of British administrators who ultimately exterminated them.[16] Their commitment to rules produced another unanticipated consequence: in the nineteenth century when some of its members became increasingly concerned with loot, the brotherhood became lax. This gave the British a unique opportunity to persuade older, more tradition-bound members that the ancient Thug belief that Kali would destroy the order when its members no longer served her required them now to help their goddess by becoming informers.

To us, a Thug is a brute, ruffian, or cut-throat, but the word originally signified deceiver, and the abilities of Thugs to deceive distiguish them radically from other related Hindu criminal associations, which also worshipped Kali but "exercised their (criminal) profession *without* disguise."[17] Thugs literally lived two very different sorts of lives, which continually amazed the British. For the greater portion of the year (sometimes 11 out of 12 months), Thugs were models of propriety, known for their industry, temperance, generosity, kindliness, and trustworthiness.

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[14] To allay Hindu anxieties concerning Thug reprisals, the British waived many rights of the defendants. Individuals could be convicted simply for being members of the group and then would be interned for the rest of their lives on grounds that they perceived Thuggee as a religious obligation and would always continue to do so. Thomas Macauley probably drew up the legislation. The rationale is explained by Hervey (1892, vol. 2, pp. 443-451 and Appendixes E and F). In World War II Gillie (1944) contended that the principles should be revived to dispose of Nazi leaders, and to some extent they were embodied at Nuremberg.

[15] No serious argument has been made that the Thugs ever had a political purpose. Russell and Hira (1916) conclude that the immunities were probably linked with Hindu concepts of luck and impurities, although the immunities may have represented tribes from which Thugs originated or disguises Thugs often assumed.

[16] Thirty to forty Europeans normally participated in these operations against some 10,000 Thugs. A few assassination attempts against officials occurred, but the assailants lost their nerve, so pervasive must have been the taboo. As far as we know, the Thugs murdered only one or two European travellers.

[17] "So far from shrinking at the appellation, when one of them is asked who he is, he will coolly answer that he is a robber" (Hutton, 1961, p. 127).

British officers who unwittingly had employed them as guardians for their children lavishly praised the reliability of Thugs who had strangled hundreds of victims. An extraordinary capacity for deception was a cardinal feature of Thug tactics too. Long journeys in India always involved great hazards, requiring parties large enough to repel attacks by marauders. Groups of Thugs disguised as travellers, sometimes numbering as many as 60 persons, were often successful in persuading legitimate travellers to join forces, thereby increasing the security of all. In some cases, the intimate congenial associations would last months before the opportunity to strike occurred. (Strangling is a difficult art and requires exceptional conditions.) Usually, close contacts of this sort create bonds between people which make cold-blooded murder difficult. In fact, the striking way in which intimacy can transform relationships between potential murderers and their victims in our own day has stimulated academics to invent a new concept—the Stockholm syndrome (Lang, 1974). But the Thugs seemed indifferent to the emotions that make such transformations possible, testifying that pity or remorse never prevented them from acting. Nonetheless, their victims were never abused. The early judicial records and interviews do not provide a single case of wanton cruelty: the victims were sacrifices, the property of Kali, and, as in all religions, the best sacrifices are those offered without blemish.[18] "A Thug considers the persons murdered precisely in the light of victims offered up to the Goddess, and he remembers them, as a Priest of Jupiter remembered the oxen and as a Priest of Saturn the children sacrificed upon the altars" (Sleeman, 1836, p. 8).

Thugs believed that death actually benefitted the victim, who would surely enter paradise, whereas Thugs who failed to comply with Kali's commands would become impotent, and their families would become either extinct or experience many misfortunes. British observers admired the cheerfulness of convicted Thugs about to be hanged, sublimely confident that they would be admitted to paradise.[19] Thugs spoke also of the personal pleasure that their particular methods generated. "Do you ever feel remorse for murdering in cold blood, and after the pretense of friendship, those whom you have beguiled into a false sense of security?" a British interrogator asked. "Certainly not. Are you yourself not a hunter of big game, and do you not enjoy the thrill of the stalk, the pitting of your cunning against that of an animal, and are you not pleased at seeing it dead at your feet? So it is with the Thug, who indeed regards the stalking of men as a higher form of sport. For you *sahib* have but the instincts of wild beasts to overcome, whereas the Thug has to subdue the suspicions and fear of intelligent men . . . often heavily guarded, and familiar with the knowledge that the roads are dangerous. Game for our hunting is defended from all points save those of flattering and cunning. Cannot you imagine the pleasure of overcoming such protection during days of travel in their company, the joy in seeing suspicion change to friendship until that wonderful moment arrives. . . . Remorse, *sahib?* Never! Joy and elation often" (Sleeman, 1839, pp. 3-4).

Assassins

The Assassins (known also as Ismailis-Nizari) survived two centuries (1090-1275). Unlike the Thugs they had political objectives; their purpose was to fulfill or purify Islam, a community whose political and religious institutions were inseparable.[20] Although by Thug standards they inflicted few casualties and wrought negligible economic damage, the Assassins seriously threatened the governments of several states, especially those of the Turkish Seljuk Empire in Persia and Syria.

As Weber (1955, p. 2) pointed out, Islam has always been preeminently dedicated to delivering a moral message aimed at transforming social existence in *this* world. Terror in Islam, therefore, has an extra dimension not present in Hinduism. The Thugs were concerned with three parties (the assailant, his victim, and a deity), but the Assassins reached out to a fourth one as well, a public or a moral community whose sympathies could be aroused by deeds that evoked attention. They did not need mass media to reach interested audiences, because their prominent victims were murdered in venerated sites and royal courts, usually on holy days when many witnesses would be present.

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[18] The prolongation of the death agony (the only exception?) was required by Thug doctrine.

[19] Apparently the major anxiety of Thugs was that they might be hung by a person of a lower caste (Spry, 1837, vol. 2, chap. 5).

[20] For the convenience of readers unfamiliar with Islamic references, I shall refer to the Nizari by their more familiar name, Assassins. When I refer to sympathetic elements, I have in mind the Shia and especially the Ismaili, the groups from which the Assassins originated. Orthodox Muslims are Sunni.

Few Assassin documents have survived, and our picture of the sect is reconstructed mostly from bitterly hostile orthodox chroniclers who obviously could not pierce the veil of secrecy, even if they had wanted to do so. Poonawala (1977) provides the most recent bibliography of sources and secondary works. Many items are annotated. The difficulties of the contemporary historian are aptly described in Hodgson (1955, pp. 22-32). Universally recognized as the best source, Hodgson's work was later sharpened (1968). My analysis is based largely on these accounts and on Lewis (1940, 1967).

To be noticed is one thing, to be understood is another, and when the object of a situation is to arouse a public, those threatened will try to place their own interpretations on the terrorist's message. Their opportunities to do so will be maximized if the assailant breaks down, or even if he tries to evade arrest. The doctrine of the Assassins seems constructed to prevent both possibilities. One who intends his act to be a public spectacle is unlikely to escape in any case. The Assassins prepared the assailant for this circumstance by preventing him from even entertaining the idea that he might survive. His weapon, which "was always a dagger, never poison, never a missile," seems designed to make certain that he would be captured or killed. He "usually made no attempt to escape; there is even a suggestion that to survive a mission was shameful. The words of a twelfth-century western author are revealing: "When, therefore, any of them have chosen to *die* in this way . . . he himself [i.e., the Chief] hands them knives which are, so to speak, 'consecrated' " (Lewis, 1967, p. 127).

Martyrdom, the voluntary acceptance of death in order to "demonstrate the . . . truth" to man, is a central, perhaps critical, method of message-giving religions, used both to dispel the doubts of believers and to aid proselytizing efforts. One cannot understand the Assassins without emphasizing the deeply embedded Muslim admiration for martyrs, particularly for those who die attempting to kill Islam's enemies. Assassin education clearly prepared assailants to seek martyrdom. The word used to designate the assailants—*fidayeen* (consecrated or dedicated ones)—indicates that they (like the victims of the Thugs) were considered religious sacrifices who freed themselves from the guilt of all sins and thereby gained "entry into paradise" (Kohlberg, 1976, p. 72).[21]

The Hindu image of history as an endless series of cycles makes Thuggee conceivable. Message-oriented religions are inclined to assume a unilinear view of history that may be fulfilled when all humans hear and accept the message. Because this aspiration is frustrated, these religions periodically produce millenarian movements predicated on the belief that an existing hypocritical religious establishment has so corrupted their original message that only extraordinary action can renew the community's faith.

Islamic millenarian movements are largely associated with the Shia (the minority), who believe that eventually a *Mahdi* (Messiah or Rightly Guided One) would emerge to lead a holy war (*jihad*) against the orthodox establishment to cleanse Islam. In the various Jewish and Christian messianic images violence may or may not appear, but "an *essential* part of the Mahdist theory regards the *jihad* in the sense of an armed revolutionary struggle, as the method whereby a perfected social order *must* be brought into being" (Hodgkin, 1977, p. 307; see also Kohlberg, 1976; MacEoin, 1982; Tyan, 1960). The believer's obligation is to keep his faith intact until the *Mahdi* summons him. To protect a believer among hostile Muslims until the moment arrives, the Shia permit pious dissimulation, *taqiyya.* The pure are allowed to conceal their beliefs for much the same reason that we condone deception during war. Should an opportunity materialize, the Shia must "use their tongues," or preach their faith openly; but not until the *Mahdi* arrives are they allowed to "draw the sword" (MacEoin, 1982, p. 121).

The Assassins apparently interpreted the injunction prohibiting swords against other Muslims to mean that the true believer could use other weapons, or perhaps even that he should do so in order to expedite the arrival of the *Mahdi.* In this respect, they resemble earlier Islamic millenarian groups, which always attached a ritual significance to particular weapons. Some eighth-century cults strangled their victims, and one clubbed them to death with wooden cudgels (Friedlaender, 1907, 1909; Watt, 1973, p. 48). In each case the weapon chosen precluded escape and invited martyrdom.

The Assassins originated from the more active Shia elements who "used their tongues," organizing missionaries or summoners to persuade fellow Muslims with respect to the true meaning of their faith. Although their roots were in Persia, many were educated in Egyptian missionary schools. When the capabilities of the Shia (Ismaili) state in Egypt to promote millenarian doctrines waned, the founder of the Assassins declared his independence, seized several impregnable mountain fortresses, and made them hospitable to all sorts of refugees. Here the Assassins developed a distinctive systematic Gnostic theology which promised a messianic fulfillment of history in a harmonious anarchic condition in which law would be abolished and human nature perfected.

Like the Thugs, the Assassins moved across state lines constantly. But the differences are important. The Thugs found it easy to make arrangements with princes who would protect them for profit and upon condition that they operate abroad. But the Assassins, aiming to reconstitute Islam into a single community again, were compelled by their doctrine to organize an international conspiracy that could not be planted in an existing Islamic state. Therefore, they had to establish their own state: a league of scattered mountain fortresses or city-states (Hodgson, 1955, p. 99).

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[21] The reference is to Shia doctrine, but it applies equally to the Assassins.

For the first time in history, perhaps, a state found its principal raison d'etre in organizing international terror. The state provided means for the creation of an efficient enduring organization that could and did recover from numerous setbacks. The earlier millenarian sodalities were too scattered, their bases were too accessible, and their consequent insignificance often made them unable to achieve even the acknowledgment of historians, which alone could make them known to us. Isolation gave the Assassins both the space and the time required to create a quasi-monastic form of life and to train leaders, missionaries, and *fidayeen.* When their popular support in urban centers evaporated after 50 years, the Assassins survived for still another century and a half and would have persisted much longer had not Mongol and Arab armies destroyed their state (Hodgson, 1955, p. 115).

To facilitate their work they organized an extensive network of supporting cells in sympathetic urban centers. Often key persons in the establishment provided internal access, support the Assassins gained through conversion, bribery, and intimidation. Since orthodox Muslims understood the importance of internal support, the Assassins manipulated apprehensions by implicating enemies as accomplices—a maneuver that multiplied suspicions and confusion.

A successful assassination policy depended upon establishing the purpose of a murder as a measure necessary to protect missionaries. Thus, one professional soldier likens the *fidayeen* to armed naval escorts, which never engage the enemy unless the convoy itself is attacked (Tugwell, 1979, p. 62). Victims were orthodox religious or political leaders who refused to heed warnings, and therefore provoked an attack by being scornful of the New Preaching, by attempting to prevent it from being heard, and by acting in ways that demonstrated complicity in Islam's corruption.

Assassin legends, like those of any millenarian group, are revealing. A most remarkable one concerned the victim-*fidayeen* relationship. Normally the movement placed a youthful member in the service of a high official. Through devotion and skill over the years he would gain his master's trust, and then, at the appropriate time, the faithful servant would plunge a dagger into his master's back. So preternatural did this immunity from personal or ordinary feelings seem to orthodox Muslims that they described the group as "hashish eaters" (*hashashin*), the source of our term assassin.(Although there is no evidence that drugs were used, the ability to use the doctrine of *taqiyya* and the fact that training began in childhood may help explain *fidayeen* behavior.) The legend is significant, too, for what it demonstrates about public responses. Everywhere Assassins inspired awe. Those favorably disposed to their cause would find such dedication admirable, whereas opponents would see it as hateful, repulsive, and inhuman fanaticism. Less obvious but much more interesting, perhaps, as a clue to responses of neutrals, is the transformation that the meaning of the term assassin underwent in medieval Europe, where initially it signified devotion and later meant one who killed by treachery (Lewis, 1967, p. 3).

The potential utility of an assassination policy is obvious. Dramatically staged assassinations draw immense attention to a cause. In the Muslim context too, the basis of power was manifestly personal. "When a Sultan died his troops were automatically dispersed. When an Amir died his lands were in disorder" (Hodgson, 1955, p. 84). When conceived as an alternative to war, assassinations can seem moral too. The assassin may be discriminating; he can strike the great and guilty, leaving the masses who are largely innocent untouched.

The problems created by an assassination policy become clear only in time. A series of assassinations must provoke immense social antagonism in the normal course of events; popular identification with some leaders will exist and assassinations themselves entail treachery. "There can be good faith even in war but not in unannounced murder. Though Muslims . . . commonly . . . used an assassination as an expedient, the adoption of . . . a regular and admitted (assassination) policy horrified them and has horrified men ever since" (Hodgson, 1955, p. 84). A similar logic moved Immanuel Kant (1948, p. 6) to describe belligerents who employ assassins as criminals; such a breach of faith intensifies hatred and diminishes the possibility of achieving a peace settlement before one party exterminates the other.[22]

As one might expect, the orthodox often responded by indiscriminately slaughtering those deemed sympathetic to the *fidayeen* (Hodgson, 1955, pp. 76-77, 111-113). The Assassins, however, reacted with remarkable restraint, eschewing numerous opportunities to reply in kind. Acts of urban terrorism occurred, the quarters of the orthodox were firebombed, but so infrequent were these incidents that one can only conclude that the rebels believed that another assassination was the only legitimate response to atrocities provoked by assassination.[23] The political consequence of this restraint was clear; after forty years, support for the Assassins among urban elements disappeared, and the massacres ceased (Hodgson, 1955, p. 115).

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[22] "A state ought not during war to countenance such hostilities as would make mutual confidence in a subsequent peace impossible such as employing assassins, poisoners, breaches of capitulation, secret instigations to treachery and rebellion in the hostile state . . . (for there must be) some kind of confidence in the disposition of the enemy even in the midst of war, or otherwise . . . the hostilities will pass into a war of extermina-

The commitment to a single, stylized form of attack is puzzling. Most of the Assassins' early millenarian predecessors found assassination attractive too, but other forms of terror were known.[24] More than any other millenarian group, the Assassins had resources to use other tactics and much to lose by failing to do so. Still, Assassin armies only protected their bases and raided caravans for booty, for it seems that Assassin doctrine made assassination and war mutually exclusive alternatives. The pattern is quite conspicuous during one of those strange periods in the movement's history when, for tactical reasons, it decided to become an orthodox community. "Instead of dispatching murderers to kill officers and divines, Hasan III sent armies to conquer provinces and cities; and by building mosques and bathhouses in the villages completed the transformation of his domain from a lair of assassins to a respectable kingdom, linked by ties of matrimonial alliance to his neighbors" (Hodgson, 1955, pp. 217-239; Lewis, 1967, p. 80). Assassin encounters with Christians also reflected the view that the dagger was reserved for those who betrayed the faith and the sword for persons who had never accepted it. When the Assassins first met invading Crusaders in Syria during the early twelfth century, they used their armies, not their *fidayeen* (Lewis, 1967, p. 108).

The peculiar reluctance to modify their tactics or to use their resources more efficiently probably had its origins, as the doctrines of all millenarian groups do, in reinterpretations of major precedents in the parent religion. To the millenarian, those precedents explain the religion's original success, and the abandonment of those precedents explains why there has been a failure to realize its promise. The life of Mohammed probably prescribed the model for Assassin strategy. The group began, for example, by withdrawing to primitive places of refuge (*dar al-hijra*), a decision that "was a deliberate imitation of that archetype from Mohammed's own career," who fled to remote but more receptive Medina when he failed to convert his own people in Mecca. "Medina was the first *dar al-hijra* of Islam, the first place of refuge—whence to return in triumph to the unbelieving lands from which one had to flee persecuted" (Hodgson, 1955, pp. 79-80). Islam's calendar dates from this event, and the pattern of withdrawing in order to begin again became one that millenarian elements in Islam normally followed and in fact do still, as recent studies of Muslim terrorist groups in Egypt show (Hodgkin, 1977; Ibrahim, 1980).

Mohammed's unusual employment of military forces and assassins while in Medina seems particularly instructive.[25] Initially, the army had only two tasks, to defend the community against attacks and to raid caravans for booty. Simultaneously, he permitted (authorized?) assassinations of prominent persons within or on the fringes of Islam, "hypocrites" (*munafikun*) who had "provoked" attacks by displaying contempt for some aspect of Mohammed's teachings. Their deaths released hitherto latent sympathies for Islam among their followers.[26] The process of

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tion. . . . Such a war and . . . all means which lead to it, must be absolutely forbidden." (*Cf.* Vattel, I, 19, 233.)

[23] The sect, of course, was the subject of many allegations, but it was never charged with instigating counter-atrocities against groups or classes. Its targets were almost invariably individuals. The sober Sunni view was that the Nizari wanted "to destroy Islam but not necessarily any . . . Muslims" (Hodgson, 1955, p. 123).

The Azraqites apparently practiced indiscriminate slaughter, arguing that every member of a family of unbelievers was an unbeliever (Watt, 1973, p. 22).

[25] The initial assassination, that of Asma bent Marwan, was occasioned by Mohammed's question, "Will no one rid me of (her)?" Henry II encouraged his knights in the same way when he grumbled about Becket. But how different the results were! Becket was martyred, the knights were punished, and the English king did penance. For a discussion of Greco-Roman and Christian attitudes toward assassination, see Rapoport (1971, chap. 1).

Six assassinations are discussed by Rodinson (1971, chap. 5). They are also described by Watt (1956), but because Watt's references are scattered throughout the text and fewer details are provided, it is more difficult to perceive patterns.

[26] In the Koran, the term hypocrite (*munafikun*) refers to those whose fidelity and zeal Mohammed could not rely upon, persons "in whose hearts there is sickness, weakness, and doubt . . . who had joined Islam perhaps reluctantly . . . usually members of the aristocracy" (Buhl, 1913). Most of those assassinated were Jews, but Mohammed's "Constitution of Medina" clearly indi-

purifying, or consolidating the original nucleus of the faith, seemed to be the precondition of expansion. When Mohammed decided the community was ready to become universal, the army was given its first offensive role and assassinations ceased!

Other aspects of the assassination pattern may have seemed suggestive too. The assassins' deeds were means to compensate or atone for deficiencies in ardor. The ability to overcome normal inhibitions or personal attachments to the victim was a significant measure of commitment. In every case, for example, assassin and victim were kinsmen, and no stronger bond was known then.[27] The victims were not likely to defend themselves (e.g., they might be asleep or be women or old men), and they were often engaged in activities likely to evoke the assailant's compassion (e.g., they were playing with children or making love). As known associates of Mohammed, the assassins could only gain access to their victims by denying their faith or denouncing the Messenger of Allah.

A major difference between the earlier assassins and the later *fidayeen* is that one group returned to Mohammed for judgment, whereas the other actively sought martyrdom. In explaining this difference, remember that the origin of the *fidayeen* is in the Shia and Ismaili sects. Those groups link themselves to Ali and Husain, whom they consider Mohammed's true heirs. Ali and Husain were themselves both martyred after authorizing assassinations, and their martyrdoms became as central to their followers as Christ's passion is to Christians.

We do not have the primary sources to determine how the Assassins actually justified their tactics, but we know they saw themselves as engaged in a struggle to purify Islam and made extraordinary efforts to demonstrate that they acted defensively. The *fidayeen* put themselves in situations in which intimate bonds or personal feelings would be violated in order to demonstrate conviction. Assassin armies had one purpose in the *hijra*; later, they were likely to have another. The precedents were well known to anyone familiar with Mohammed's life and with the lives of figures most central to the Shia. Can there be justifications more compelling for believers than those that derive directly from the founders of their faith?

Zealots-Sicarii[28]

There are resemblances between the Assassins and the Zealots-Sicarii. Both were inspired by messianic hopes to seek maximum publicity. Both

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cates that his original community included Jews, and initially he intended to bring Islam as close as possible to Judaism. When that policy failed, the assassinations were an essential aspect of the struggle to separate the two religious bodies and to gain converts out of the Jewish tribes. The process is illustrated in the aftermath of the first assassination, that of a Jewish poetess by 'Umayr, her kinsmen: " 'Umayr returned to his own clan, which was in a great uproar. Decide what is to be done with me, but do not keep me waiting! No one moved. . . . That was the day when Islam first showed its power over the Banu Katma. 'Umayr had been the first among them to become a Muslim. On the day the daughter of Marwan was killed, the men of the Banu Khatma were converted because of what they saw of the power of Islam" (Ibn Hisham quoted by Rodinson, 1971, p. 171).

[27] Margoliouth (1923, p. 116) notes that Muslim initially meant "traitor, one who handed over his kinsmen or friends to their enemies," and that "Mohammed . . . displayed great ingenuity" in transforming its meaning into "one who handed over his own person to God." The new religion, he believes, could not survive without challenging the kin bond; and "Islam, as appears from the most authorized traditions, had the effect of making men anxious . . . to signalize their faith by parricide or fratricide" (p. 265). The traditional or orthodox interpretations of these incidents is that the assailants, shamed by their kinsmen's behavior, acted on their own initiative.

[28] No terrorist campaign before the nineteenth century is better known, and virtually all our information comes from Josephus Flavius, a Jewish commander who later became a Roman supporter and portrays the Zealots and *Sicarii* as provoking the popular uprising when no irreconcilable issues divided Roman and Jew. How reliable is Josephus? Historians have always disagreed. He has been seen as a "mere Roman apologist," and the accounts he challenges have vanished. His description, like those of all ancient historians, wildly exaggerates statistics and contains inconsistencies which serve explicit didactic purposes. Still, moderns increasingly find him credible, except on particular matters where good reason to mistrust him exists. When his sources can be checked, he "remains fairly close to the original. Even when he modifies the source to suit a certain aim, he still reproduces the essence of the story. More important, he does not engage in the free invention of episodes . . . like other (ancient) authors. . ." (Cohen, 1979, p. 233). All other extant sources, Roman and Jewish materials alike, are more hostile to the rebels than Josephus himself was. Although some say "that Josephus' good faith as a historian cannot be seriously questioned" (Shutt, 1961, p. 123), most agree that despite other concerns he truly had "an interest as a historian in the course of events themselves" (Bilde, 1979, p. 201).

The second issue is which of Josephus' different and contradictory assessments of motives is most credible? I have followed the modern tendency in playing down the criminal and personal motives Josephus gives to the rebels in order to emphasize their religious and political concerns. And I have taken seriously his frequently interpreted important events in the founding period of their religion as precedents for their tactics and to mean also that those who died in this struggle secured their places in paradise. Like the Assassins, the Sicarii (daggermen) were identified with a particular weapon, and both rebellions had an international character. Nonetheless, the differences between the two, which derive from variations in the content of their respective messianic and founding myths, are even more striking.

The Zealots-Sicarii survived for approximately 25 years, a brief existence by the standards of the Assassins, but their immediate and long-run influence was enormous. Holy terrorists are normally concerned with members of their own religious culture, but the Jews were also interested in generating a mass uprising against the large Greek population that lived in Judea and against the Romans who governed them both. The revolt proved disastrous and led to the destruction of the Temple, the desolation of the land, and the mass suicide at Masada. Moreover, Zealot-Sicarii activities inspired two more popular uprisings against Rome in successive generations, which resulted in the extermination of the large Jewish centers in Egypt and Cyprus, the virtual depopulation of Judea, and the final tragedy—the Exile itself, which exercised a traumatic impact on Jewish consciousness and became the central feature of Jewish experience for the next two thousand years, altering virtually every institution in Jewish life. It would be difficult to find terrorist activity in any historical period which influenced the life of a community more decisively.

The impact of the Jewish terrorists obviously stems from their ability to generate popular insurrections, an unusual capacity among religious terrorists which makes them particularly interesting to us because ever since the Russian Anarchists first created the doctrine of modern terror, the development of a *levee-en-masse* by means of provocation tactics has been the principal aim of most groups. Very few have succeeded, and none has had as much success as the Zealots-Sicarii did. Why were they so peculiar?

The nature of their messianic doctrines simultaneously suggested the object of terror and permitted methods necessary to achieve it. Jewish apocalyptic prophecies visualize the signs of the imminence of the messiah as a series of massive catastrophes involving whose populations, "the upsetting of all moral order to the point of dissolving the laws of nature" (Scholem, 1971, p. 12). This vision saturated Judaism for a generation preceding the genesis of Zealot-Sicarii activity, creating a state of feverish expectancy. "Almost every event was seized upon . . . to discover how and in what way it represented a Sign of the Times and threw light on the approach of the End of the Days. The whole condition of the Jewish people was psychologically abnormal. The strongest tales and imaginings could find ready credence" (Schonfield, 1965, p. 19). New messianic pretenders flourished everywhere, because so many people believed that the signs indicating a messianic intervention were quite conspicuous: Judea was occupied by an alien military power, and prominent Jews were acquiescing in "the desecration of God's name" or accepting the culture of the conqueror.

In all apocalyptic visions God determines the date of the redemption. Still, these visions often contain some conception that humans can speed the process. Prayer, repentance, and martyrdom are the most common methods. When these do not produce results and a period of unimaginable woe is perceived as the precondition of paradise, it will only be a matter of time before believers will act to force history, or bring about that precondition. Jewish terrorist activity appeared to have two purposes: to make oppression so intolerable that insurrection was inevitable, and, subsequently, to frustrate every attempt to reconcile the respective parties.

The names Zealot and Sicarii both derive from a much earlier model in Jewish history, Phineas, a high priest in the days of Moses. His zeal or righteous indignation averted a plague that afflicted Israel when the community tolerated acts of apostasy and "whoring with Moabite women." Taking the law into his own hands, he killed a tribal chief and his concubine who flaunted their contempt for God in a sacred site. Phineas is the only Biblical hero to receive a reward directly from God (*Numbers* 25:11). In purifying the community, his action prepared the way for the Holy War (*herem*) which God commanded Israel to wage against the Canaanites for the possession of the Promised Land. The Bible repeatedly refers to

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repeated contention, which some scholars question, that the terrorists forced their will on reluctant parties. The process of polarizing a society by exploiting latent hostilities through shock tactics was not understood well by the nineteenth- and early twentieth-century commentators on Josephus who knew of no terror campaigns with which to compare the revolt. More recent scholars display less skepticism on this point. My earlier study (1982b) is a step-by-step analysis of the dynamic presupposed by Josephus' account, and the description above is based upon that essay.

The literature on the revolt is quite extensive. The following articles (in addition to those cited above) were particularly helpful: Applebaum (1971), Betz et al. (1974), Borg (1971), Farmer (1956), Grant (1973), Hengel (1961), Horsley (1979b), Kingdom (1970, 1971), Kohler (1905), Roth (1959), Smallwood (1976), Stern (1973), Thackeray (1967), and Zeitlin (1967).

the terror that the *herem* was supposed to produce and to Israel's obligation to destroy all persons with their property who remain in the land, lest they become snares or corrupting influences. The word *herem,* it should be noted, designates a sacred sphere where ordinary standards do not apply, and in a military context, a *herem* is war without limits.[29]

The name Sicarii comes from the daggers (*sica*) used when the group first made its appearance. Rabbinic commentary indicates that Phineas used the head of his spear as a dagger, and the Sicarii normally assassinated prominent Jews, especially priests, who in their opinion had succumbed to Hellenistic culture. As in Phineas's case, these acts were also efforts to create a state of war readiness, and, more specifically, to intimidate priests who were anxious to avoid war with Rome and whose opposition could prevent it from materializing.

> The Sicarii committed murders in broad daylight in the heart of Jerusalem. The holy days were their special seasons when they would mingle with the crowd carrying short daggers concealed under their clothing with which they stabbed their enemies. Thus, when they fell, the murderers joined in cries of indignation, and through this plausible behavior, were never discovered. The first assassinated was Jonathan, the high-priest. After his death there were numerous daily murders. The panic created was more alarming than the calamity itself; everyone, as on the battlefield, hourly expected death. Men kept watch at a distance on their enemies and would not trust even their friends when they approached (Josephus, 1926a, vol. 2, pp. 254-257).

Although their name reminds us of Phineas's weapon, his spirit and purpose were more decisive influences. Unlike the *fidayeen,* the Sicarii did not limit themselves to assassinations. They engaged military forces openly, often slaughtering their prisoners. They took hostages to pressure the priests and terrorized wealthy Jewish landowners in the hopes of compelling a land redistribution according to Biblical traditions. The Zealots illustrate the point even more clearly. Their Hebrew name signified the righteous indignation that Phineas personified, but they rarely plotted assassinations, and their principal antagonists were non-Jews who dwelled in the land. Phineas was also known for his audacity, which Zealot-Sicarii assaults often reflect. (It is not without interest that rage and audacity are qualities most admired and cultivated by modern terrorists.) Their atrocities occurred on the most holy days to exploit the potential for publicity therein, and, more important, to demonstrate that not even the most sacred occasions could provide immunity. Note, for example, Josephus's description of how the Sicarii massacred a Roman garrison, after it had secured a covenant (the most inviolable pledge Jews could make) that guaranteed the troops safe passage.

> When they had laid down their arms, the rebels massacred them; the Romans neither resisting, nor suing for mercy, but merely appealing with loud cries to the covenant! . . . The whole city was a scene of dejection, and among the moderates there was not one who was not racked with the thought that he should personally have to suffer for the rebels' crime. For to add to its heinousness the massacre took place on the sabbath, a day on which from religious scruples Jews abstain from even the most innocent acts (Josephus, 1926a, vol. 2, p. 451).

The massacre electrified the Greeks, who constituted a significant portion of the population in Judea and were the local source of Roman recruitment. Jews in numerous cities were massacred, and everywhere the Greeks were repaid in kind. The action and the response illustrate vividly some salient differences between Muslim and Jewish terrorists. *Fidayeen* terror was an auxiliary weapon designed to protect their missions where the main work of the movement was done, converting the population to a particular messianic doctrine. Patient and deliberate, the Assassins acted as though they expected to absorb the Muslim world piecemeal. The Zealots and the Sicarii saw themselves not as the propagators of a doctrine but as revolutionary catalysts who moved men by force of their audacious action, exploiting mass expectations that a cataclysmic messianic deliverance was imminent.

To generate a mass uprising quickly and to sustain constantly increasing polarizing pressures, the Zealots-Sicarii developed an array of tactics unusual by Thug and Assassin standards. Participants (despite their contrary intentions) were pulled into an ever-escalating struggle by shock tactics which manipulated their fear, outrage, sympathy, and guilt. Sometimes these emotional affects were provoked by terrorist atrocities which went beyond the consensual norms governing violence; at other times they were produced by provoking the enemy into committing atrocities against his will.

Thugs and Assassin tactics always remained the same, but in the different phases of the Jewish uprising, striking changes occurred which seemed designed for specific contexts. The rebellion began with passive resistance in the cities. This tactic, of which the Jewish example may be the earliest recorded by historians, merits comment, for in our world (e.g., Cyprus and Northern Ireland), passive resistance has often appeared as an initial step in conflicts which later matured into full-scale terrorist campaigns.[30] Our experience has been that many who would have shrunk from violence, let alone terror, often embrace passive resistance as a legitimate method to rectify grievances, without understanding how the ensuing drama may intensify and broaden commitments by simultaneously exciting hopes and fanning smouldering hostilities.

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[29] For a convenient discussion of the *herem* and its revival by the Zealots-Sicarii as reflected in the Dead Sea Scrolls, see de Vaux (1972, pp. 258-267). The later conception had new elements: the war would be a war to end all wars, it would involve all men, and the enemy was under Satan's influence.

In the Jewish case, before antagonisms had been sufficiently developed and when Roman military strength still seemed irresistible, passive resistance might have been the only illegal form of action that many Jews would willingly undertake. Initially, the confrontations involved Jewish claims, sometimes never before made, for respect due to their sacred symbols, and governments learned that, willy-nilly, they had backed, or been backed, into situations in which they either had to tolerate flagrant contempt for the law or commit actions that seemed to threaten the Jewish religion, the only concern that could unite all Jews. More often than one might expect, the Romans retreated in the face of this novel form of resistance. They admired the Jews' displays of courage, restraint, and intensity, and they learned how difficult and dangerous it was to break up demonstrations that included women and children (Josephus, 1926a, vol. 2, pp. 169, 195; 1926b, vol. 18, pp. 55, 269). They feared a rebellion that could engulf the eastern portion of the Empire, which was at least one-fifth Jewish and contained a significant class of Jewish sympathizers (*sebomenoi,* God-fearers) whose influence seemed to reach members of Rome's ruling circles.

The possibility that the conflict could become an international one troubled Rome. Judea was on the frontier next to Parthia, the last remaining major power in the ancient world. Parthia had intervened in earlier conflicts. Even if Parthia wanted to avoid involvement, she might find it difficult to do so because her Jewish population was large, and one Parthian client state had a Jewish dynasty that bore a special hatred for Rome. Parthian Jews were important figures in the early stage of the rebellion. The great annual pilgrimages of Parthian Jews to Jerusalem and the massive flow of wealth they contributed to maintain the Temple gave evidence of the strength of their tie to Judea, a bond that a modern historian compares to that which knitted American Jews to those in Palestine during the uprising against Britain.

For some time before the rebellion, Rome kept expanding the unusual exemptions given Jews, and the uprising was fueled partly by rising expectations. But Rome's anxiety to avoid a serious conflict simply made her more vulnerable to tactics calculated to produce outrage. Her restraint encouraged reckless behavior and weakened the case of Jewish moderates who argued that although Rome might be conciliatory, she was wholly determined to remain in Judea.

Large passive demonstrations against authority tend to produce violence unless both sides have discipline and foresight. When some on either side prefer violence, or when passive resistance is viewed not as an end in itself, but as a tactic that can be discarded when other tactics seem more productive, explosions will occur. Whatever the particular reason in this case, demonstrators soon became abusive, and bands of rock-throwing youths broke off from the crowds. When Roman troops (trying to be inconspicuous by discarding military dress and exchanging swords for wooden staves) were attacked, Roman discipline dissolved. The crowds panicked, and hundreds of innocent bystanders were trampled to death in Jerusalem's narrow streets. This pattern kept repeating itself, and the atrocities seemed especially horrifying because they normally occurred on holy days when Jerusalem was crowded with pilgrims, many of whom were killed while attending religious services. The massive outrage generated by Roman atrocities and the assassination campaign against the moderates finally intimidated reluctant priests into refusing to allow Roman sacrifices at the Temple. Rome viewed that act as a rejection of her sovereignty or as a declaration of war, and this gave the militants a plausible case that the war was indeed a *herem.*

When the war finally occurred, many on both sides hoped to conclude it quickly with a political settlement. These hopes were given a severe jolt early after the first military engagement. When the tiny Roman garrison in Jerusalem, which had laid down its arms for a covenant of safe passage, was massacred, a pattern of reprisal and counter-reprisal spread throughout the eastern portion of the Empire. Roman troops ran amuck. Yet when military discipline was finally restored, the Roman campaign quite unexpectedly was restrained. Military advantages were not pressed, as hope persisted that the olive branch offered would be seized. Understanding that most Jews wanted peace, Rome believed that the atrocities of Jew against Jew would eventually destroy the popular tolerance requisite for all terrorist movements. A significant Jewish desertion rate, including many important personalities, kept Roman hopes alive for negotiating a peace without strenuous military efforts. But various Jewish atrocities, which culminated in the cold-blooded murder of Roman peace envoys, led to the conclusion that only total war was feasible (Josephus, 1926a, vol. 2, p. 526).

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[30] Rapoport (1982b, pp. 36-37) discusses relationships between the process described here and modern campaign experiences. For a general discussion of passive resistance and terrorism, see Thornton (1964, p. 75).

Zealot-Sicarii strategy seemed admirably designed to provoke a massive uprising. Consecutive atrocities continually narrowed prospects for a political, or mutually agreeable, solution, serving to destroy the credibility of moderates on both sides while steadily expanding the conflict, which enlisted new participants. But no master hand can be detected in this process, and one can see it as an irrational process. Jewish terrorists reflect a bewildering assortment of forces. Several Zealot and at least two Sicarii organizations existed, and many other groups participated, but only a few can be identified. Then, as now, the effect of multiplicity was to encourage each element toward even more heinous atrocities, in order to prove the superiority of its commitments, and in time the groups decimated each other. As these extraordinary actions unfolded, the participating groups, like so many of their modern counterparts, found it necessary to make even more fantastic claims about their enemies and even more radical promises about the social reconstruction that would result from their victory. Ferrero's comment on the dynamics of the French Reign of Terror seems quite pertinent. "The Jacobins did not spill all that blood because they believed in popular sovereignty as a religious truth; rather they tried to believe in popular sovereignty as a religious truth because their fear made them spill all that blood" (1972, p. 100; cf. Josephus, 1926b, vol. 18, p. 269).

To focus on popular insurrection as the principal object, however, is to misconstrue the Zealot-Sicarii view. Insurrection was only a sign of messianic intervention, and because they were concerned with a divine audience, they did things that no one preoccupied with a human audience alone would dream of doing. The decision of Zealot leaders to burn the food supply of their *own* forces during Jerusalem's long seige becomes intelligible only if one believes that He might see it as proof that the faithful had placed all their trust in Him. God, therefore, would have no choice; was He not bound by His promise to rescue the righteous remnant? Because many thought God would be moved by their sufferings, the most profound disaster often created new hopes. When the Temple was burning (and the war irretrievably lost), a messianic impostor persuaded six thousand new recruits that the fire signified that the time for deliverance had finally arrived. Compared to the Thugs and Assassins, the Zealots-Sicarii seem free to choose their tactics, but how can one be free to follow an impossible goal?

Conclusion

These cases provide materials to broaden the study of comparative terrorism. Each contains parallels worth pondering, and the three together illustrate the uniqueness of sacred terror and thus provide a perspective for viewing modern terror and a glimpse of the latter's special properties.[31]

Our obliviousness to holy terror rests on a misconception that the distinction between it and the modern form is one of scale, not of nature or kind. A most conspicuous expression of this misconception is the conventional wisdom that terrorist operations require modern technology to be significant. There are relationships between changes in technology and changes in terrorist activity, but they have not been seriously studied. More important, every society has weapon, transport and communication facilities, and the clear meaning of our cases is that the decisive variables for understanding differences among the forms terror may take are a group's purpose, organization, methods, and above all the public's response to that group's activities.

This conclusion should shape our treatment of the dynamics of modern terrorism. There is no authoritative history of modern terrorism that traces its development from its inception more than a century ago. When that history is written, the cyclical character of modern terror will be conspicuous, and those cycles will be related not so much to technological changes as to significant political watersheds which excited the hopes of potential terrorists and increased the vulnerability of society to their claims. The upsurge in the 1960s, for example, would be related to Vietnam just as the activities immediately after World War II would appear as an aspect of the decline in the legitimacy of Western colonial empires. Since doctrine, rather than technology, is the ultimate source of terror, the analysis of modern forms must begin with the French, rather than the Industrial Revolution.

When the assumption concerning technology is abandoned, early cases seem more valuable as a source for appropriate parallels. We have already suggested a number of potentially instructive instances. For example, the Zealot-Sicarii case may be the only instance of a successful strategy that actually produced a mass insurrection—the announced objective of modern revolutionary terror. It illuminates predicaments inherent in this strategy while exposing aspects of societies especially vulnerable to it. It is worth noting that the problems illustrated by this particular experience concerned Menachem Begin greatly, because his strategy as the leader of the Irgun in the uprising against Britain was in part conceived to avoid "mistakes" made by the Zealots-Sicarii (Rapoport, 1982b, pp. 31-33).

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[31] A third reason for studying sacred terror is that there are direct links between some of its concepts and those that animate modern forms (Dugard, 1982).

The international context provides another parallel. It played a crucial role in sustaining the terror. The Thugs and Assassins had valuable foreign sanctuaries. Favorable, albeit different, international climates of opinion helped all three groups. In each case there was cooperation among terrorists from different countries; in one instance a state was actually directing an international terrorist organization, and in another there existed the threat of potential military intervention by an outside power. The problems posed and the constraints involved provide useful points of comparison with modern experiences. The difficulties in dealing with terrorists who have foreign sanctuaries and the ways in which those difficulties may exacerbate international relations are familiar. Rome's vulnerability to terror tactics reminds one of Western colonial empires after World War II, but the ultimate reason for the different outcomes was that Rome never doubted her right to rule. Britain's ability to exterminate the Thugs quickly in the nineteenth century was to a large extent the consequence of a favorable British and an indifferent international opinion. Perhaps the doubt expressed in the 1930s by a student of the Thugs that Britain could not have acted as decisively to deal with the same problem a century later was unwarranted, but the concern reflected a very different political environment, one that is even more deeply rooted today.[32]

How should we characterize sacred terror? Obviously there are enormous variations in its expressions which extend to purposes, methods, responses, and differences that derive from the ingenuity of the individual terror cult which in turn is limited by boundaries established by the original religion. In an odd, interesting way, the terrorist as a deviant highlights unique features of the parent religion that distinguish it from other religions, e.g., concepts of the relation of the divine to history and to social structure.

Because Hinduism provides no grounds for believing that the world can be transformed, the Thugs could neither perceive themselves nor could they be perceived as rebels. In imagining themselves obligated to keep the world in balance, they were part of the established order, though obviously not in it. In Islam and Judaism, the potentialities for radical attacks on institutions are inherent in the ambiguity of unfulfilled divine promises, which no existing establishment can reconcile fully with its own dominance. Because the promises are known to every member of the religious community, the Islamic or Jewish terrorist has a human audience not present in Hinduism. To reach this audience Islamic and Jewish terrorists must become visible and must either conquer all or be extinguished. There can be no such imperative with respect to the Thugs, as the extraordinarily long life of the order suggests. Initially the British were very reluctant to suppress the Thugs because they believed that it would be dangerous to disturb the local and foreign interests embedded in Thug activity. The decisive impetus was a rekindling of evangelical feeling in Victorian England which struck out at the world slave trade and was outraged by accounts of three ancient Hindu practices: infanticide, immolation of widows, and Thuggee. Under Hindu administration, Thuggee would have survived much longer.

If a particular religion creates boundaries for its terrorists, it follows that similarities within traditions will be as striking as differences among traditions. In the Hindu world, an ancient species of criminal tribes, all of which worshipped Kali, persisted. Each performed a particular criminal vocation, was committed to a special way of achieving it, and believed that its actions were legitimate by Hindu standards. The Thugs were unique among those tribes in not professing their practices openly; perhaps they could not have been able to survive the outrage and horror provoked by them. The Assassins' situation is more straightforward; they were the latest and most successful Muslim millenarian assassination cult and the only one that established a state, the mechanism required for thorough organization. The Assassins consummated a millenarian tradition of terror, but the Zealots-Sicarii appeared to have initiated one, which ended after three disastrous massive revolts in as many generations. Holy terrorists normally victimize members of the parent religion, but the Jews attacked non-Jews too, those who resided in the land. The concern with the land as the site of the messianic experience may be a distinguishing feature of Jewish terror. The conception of a war without limits in which large military forces are engaged probably had its roots in the extraordinary Holy War (*herem*), which, according to the Bible, God Himself authorized in the original conquest of Canaan. The belief that assimilation impeded messianic deliverance and that all members of the community were culpable gave Jewish terror a character that seemed indiscriminate, certainly by the standards of the Assassins, who held leaders responsible.

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[32] In 1933, J. L. Sleeman wrote, "it is of interest to speculate as to what the procedure would be today were such an organization of murder to be discovered in India, and imagination runs riot at the long vista of Royal Commissions, Blue, Red, and White Books, Geneva Conferences and the political capital which would be made of it, the procrastination and the delay, tying the hands of those on the spot, and the world propaganda which would ensure. . . . Thuggee could shelter behind disunited party government" (p. 103).

Sacred terrorists find their rationale in the past, either in divine instructions transmitted long ago or in interpretations of precedents from founding periods of the parent religions. Their struggles are sanctified with respect to purpose and with respect to means; this is why their violence must have unique characteristics. The very idea of the holy entails contrast with the profane, the normal, or the natural. The noose of the Thug and the dagger of the Assassin illustrate the point. It is difficult, in fact, to avoid feeling that the act of terror is holy just because one is acting against his natural impulses. The immunities of Assassins and Thugs to natural feelings (i.e., the Stockholm syndrome) astonished observers. But, unlike terrorists we are familiar with, they began training for their tasks as children. Our sources provide no information on the personal stress that the methods of the Jewish terrorists might have created for them, but perhaps it is relevant that the Bible relates instance after instance of individuals, including King Saul himself, who violate commands for indiscriminate destruction in the original *herem* to conquer Canaan.

Religion normally embodies ritual, and it does seem natural that rules prescribe every detail of Hindu and Islamic terror. As observers of the Thugs pointed out, those rules may have been rationally designed to resolve perennial practical problems, thus helping the groups to endure and become more effective. Still, divinely authorized rules cannot be altered even when they become destructive. So conspicuous were the Assassins' political concerns that an eminent historian has described them as the first to use "political terror" in a "planned systematic fashion" (Lewis, 1967, p. 269); but their religious mandate kept them committed to the same tactics even when they proved politically counterproductive. Jewish terror appears unique, being thoroughly antinomian and embracing a large variety of activities. The success in provoking insurrection and the freedom regarding means suggest that political considerations were paramount. But since their ultimate concern was to create *the* catastrophe that would compel God to redeem the righteous remnant, in the end they, like the Thugs and Assassins, continued to act in manifestly self-destructive ways.

The transcendent source of holy terror is its most critical distinguishing characteristic; the deity is perceived as being directly involved in the determination of ends and means. Holy terror never disappeared, and it seems to be reviving in new forms especially in, but not exclusive to, the Middle East. Still, modern terror, which began initially in the activities of *Narodnaya Volya,* a nineteenth-century Russian organization, now is much more common. The modern terrorist serves political ends to be achieved by human efforts alone, and he, not God, chooses the most appropriate ends and means. It is also true that modern terrorist organizations (especially the most durable and effective ones) are often associated with religious groups, for religion can be a major factor of ethnic identity. Although the IRA attracts Catholics and repels Protestants, its object is political, and no member believes that God participates in the struggle. The FLN in Algeria stressed its Muslim character, and EOKA in Cyprus was affiliated with the Greek Orthodox church, but the tactics in both cases were designed to appeal to various domestic and international audiences.

When the members of *Narodnaya Volya,* the first modern rebel terrorists, began their activities, they seemed to be engaged in a kind of sacred ritual. More specifically, they remind one of the Assassins. Highly ranked officials who symbolized the system and bore some responsibilities for its injustices were the victims, and the assailant hoped to attract moral sympathy through his own suffering, specifically by his willingness to accept death in a public trial where he could indict the system. Even his weapon—a hand-thrown bomb—suggests the *fidayeen*'s dagger because it forced face-to-face encounters virtually precluding escape, which persuaded many observers that his will to die was more compelling than his desire to kill (Ivianski, 1982). But, unlike the Assassins, the possibility of other terror tactics was visualized early by their contemporaries, and their initial patterns were soon discarded.

Modern terrorism has two unique, dominant features. Organizations and tactics are constantly modified, presumably to enhance effectiveness, and terror is used for very different ends, ranging from those of anarchists with millenarian visions, to anti-colonialists, to individuals who simply want to call attention to a particular situation that they find offensive. The early forms of sacred terror cannot be characterized this way. The ends are predetermined, and no real evidence exists that the participants learn to alter their behavior from others within their own tradition, let alone from those outside it.[33] Modern terrorists take their lessons from anyone, and in an important sense they constitute a single tradition which reflects and caricatures a much-observed tendency in our world to subject all activities to efficiency tests. Over the decades the tendency has been to choose methods that minimize the terrorist's risks; the targets, accordingly, are, increasingly, defenseless victims who have less and less value as symbols or less and less responsibility for any condition that the terrorists say they want to alter. The question is whether one can place a premium on reducing the assailant's risk without undermining his potential impact. The problem did not exist for the sacred terrorist, which may be one reason why he was so effective.

The desire to make terror "rational" dominated the first modern terrorist text, Nechaev's *Revolutionary Catechism,* produced before the birth of *Narodnaya Volya.* "The revolutionary (terrorist) . . . knows only one science: the science of destruction. For this reason, and only for this reason, he will study mechanics, chemistry, and perhaps medicine. But all day and night he studies the living science of peoples, their characteristics and circumstances, and all the phenomena of the present social order. The object is the same. The prompt destruction of this filthy order" (1971, p. 71). Nechaev's work is simply an exercise in technique, suggesting devices for provoking governments to savage their peoples until the latter can bear it no longer. It has had numerous successors, the latest and most notorious being Marighella's *Minimanual of the Urban Guerrilla.*

Although the disposition to apply standards of expediency distinguishes modern from holy terror, the presence of this disposition itself cannot mean that modern terrorists are rational. Some ends in principle may be impossible to achieve, like those of the anarchist; others may be so ill-considered that no means can be made rational—the situation, it seems, of the Baader Meinhoff group and the Italian Red Brigades. Sometimes, under the guise of expediency, the safety of the terrorist might become the prime concern. More fundamentally, the very idea of a rational or expedient terror may be contradictory, since by definition terror entails extranormal violence, and as such, is almost guaranteed to evoke wild and uncontrollable emotions. Indeed, the people attracted to it may be so intrigued by the experience of perpetrating terror that everything else is incidental.

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[33] The Crusades are the major exception, for they were inspired by the *herem* and undertaken to regain the Holy Land in order to initiate a messianic era.

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The Performance of Rational Voter Models in Recent Presidential Elections

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In An Economic Theory of Democracy, *Downs hypothesized that rational, utility-maximizing citizens would calculate the benefits from voting (as opposed to abstaining altogether) and then vote if this expected benefit exceeded voting cost. This hypothesis is logically derived from economic principles of rational behavior, but is counterintuitive: even if the utility increment is plausibly large, the probability of casting a pivotal, tie-breaking vote in any sizable polity is minute. Hence the expected benefit will be less than the time cost of voting, and no rational person will vote. Nevertheless, a number of statistical studies have found that voter turnout does respond to* differences *in the probability of casting a decisive vote, even though that probability may be small.*

This article uses data gathered at the state level from the 1968, 1972, 1976, and 1980 general elections to reestimate the relationship between voter turnout and the probable closeness of the election. Pooled data Least Squares Dummy Variable (LSDV) regressions are used whenever preliminary tests indicate that such pooling is permissible. The empirical results suggest that the relationship between closeness and turnout is weak, unstable, or nonexistent in all of the models tested.

Much of the controversy in the literature on rational voting behavior in general elections revolves around the measurement and relevance of the voting power of the individual citizen. This voting power is often related directly or indirectly to the probability, *P*, that one additional vote can affect an election outcome. The first section reviews some of the issues in this controversy. The second section provides mathematical models of *P*, based on the binomial probability distribution, followed by a description of the specification and results of four models used in previous statistical tests of the rational-voter or election-closeness hypothesis—the theory that voter turnout is likely to be higher in districts where the election outcome is expected to be a near tie, because voters respond rationally to the large perceived value of *P*. The fourth section evaluates the performance of these and other models in explaining voter turnout by state in the four presidential elections since 1968. The remainder of the article is devoted to a discussion of statistical problems, a summary of findings, and a note on the problem of declining voter participation rates.

Voting as a Rational Decision

Political scientists, economists, and other public choice theorists have long been concerned with explaining voter behavior. One approach has been to examine why some eligible citizens vote in public elections, whereas others abstain entirely. In *An Economic Theory of Democracy,* Downs (1957) argues that citizens will rationally weigh the costs and expected benefits of voting versus not voting and will cast a ballot only if the benefits exceed the cost. This model of potential-voter behavior can be expressed as $R = PB - C$, where R is the expected net reward earned by a citizen by going to the polls, B is the incremental benefit or utility that accrues to the citizen when his or her preferred candidate, party, or alternative wins the election, and P is the probability that the citizen's vote will alter the election outcome. Downs identifies the cost of voting, C, as the opportunity cost of time and resources spent in voting and in acquiring and assimilating information needed to cast an informed vote. According to this model, if R is positive, the citizen will vote.[1] By implication, voter turnout should be higher in districts where

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[1] Tullock (1967) also develops a model of voter participation based on precepts of rational behavior. Ferejohn and Fiorina observe that "Downs and Tullock model the voting decision as an investment decision. For the prospect of some uncertain future benefit (*PB*) one accepts costs (*C*) with certainty in the present" (1974, p. 526).

the election is expected to be close, because P will be higher.

Downs recognizes that P is likely to be small and infers that most citizens will not spend much effort on acquiring information: "But in fact [the citizen's] vote will not be decisive: it is lost in a sea of other votes. Hence whether he himself is well-informed has no perceptible impact on the benefits he gets" (1957, p. 246). But other students of this field have gone on to argue that because P is so small, if there is any cost of voting whatsoever, R will almost certainly be negative, and a citizen would be irrational to vote. As Stigler writes: "The so-called voter's paradox is simply that rational conduct will normally lead to non-voting, yet the majority of the population . . . votes" (1972, p. 103).[2]

Because this is a rather grim conclusion—either many principles of rational behavior don't apply to a large area of public choice decisions, or else democracy only works as advertised when citizens are irrational—the analysis has been extended in several ways. Riker and Ordeshook (1968) treat the Downsian expected utility model as $R = PB + D - C$, where D is the direct benefit a citizen receives by voting.[3] Ferejohn and Fiorina (1974) build a "minimax regret" model, in which a citizen votes to avoid the worst possible election outcome: the preferred candidate loses by one vote. Hinich (1981) argues that voters derive utility from contributing to a winner's campaign, and voting is the smallest contribution; there is therefore a reward from voting even if the individual's vote is not decisive. According to these views, it is rational to vote even if the election is not expected to be close. On the other hand, Brunk demonstrates that voter participation is reduced in groups that have been exposed to the Downs model and to the idea that P is very small. He writes: "The reason why so many people vote is because democratic societies systematically feed their citizens false information about the utility each individual personally gains from the franchise" (1980, p. 550). Since it costs very little to vote, people without much knowledge of rational voting models are rational if they defer to social norms and vote.[4]

Mention should be made of a hybrid rationale for voting which is rational in an instrumental sense, but which need not depend on the probable closeness of an election outcome. Suppose an individual belongs to a demographic group that is politically distinct in the sense that, on some issues, the group might vote nearly *en bloc*. If the group can break ties in strategic districts, candidates and parties will tend to adopt policies and platforms acceptable to the group before the election. In such circumstances, the actual winner of the election becomes somewhat less important to the group's members. But for the group to continue to represent a credible threat (i.e., to continue to exert pre-election influence on programs), it must at minimum provide consistently high voter turnouts. It is therefore rational for individual group members to vote and to persuade others to do likewise, even if the immediate pre-election polls forecast a landslide victory for one side or the other. Voting is an investment in future political clout, amortized possibly over several future elections. This instrumental voting hypothesis would predict higher voter participation rates in districts where internally cohesive voting blocs constitute a larger proportion of the electorate.[5]

The Probability of Casting a Tie-Breaking Vote

The Binomial Probability Approach

Let N be the number of eligible voters in some election district and n be the (even) number who will actually vote. The ballot contains two alternatives, Democrat and Republican; ρ is the proportion of N who favor the Republican. Then P, the probability that the n voters will effect a tie, can be treated as the binomial probability that $n/2$ voters choose the Republican:

$$P = \frac{n!}{[(n/2)!]^2}\,\rho^{n/2}\,(1-\rho)^{n/2} \qquad (1)$$

By Stirling's formula, $n! \approx n^n e^{-n}\sqrt{2\pi n}$ when n is large, and

$$P = p(n,\rho) \approx \frac{2^{n+1}}{\sqrt{2\pi n}}\,[\rho(1-\rho)]^{n/2} \qquad (2)$$

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[2] See also Barry (1970, p. 18) and the Kenneth Arrow citation in Barzel and Silberberg (1973).

[3] The D term was recognized by Downs, but stressed by Riker and Ordeshook. For a critical comparison of both approaches, see Barry (1970).

[4] Thompson (1982) reminds us of the distinction between normative and descriptive decision models, and favors the Downsian expected utility model (over the minimax regret specification) for explaining observed voter turnout. For other views on the expected utility model, see Black (1980) and Filer and Kenny (1980).

[5] The increased turnouts will also be due, in part, to the heightened political awareness of *all* district residents as campaigners target the district for increased media and other expenditures.

For fixed n, P is maximized at $\rho = ½$; $\delta P/\delta n \leqslant -P/2n \leqslant 0$.

According to rational voter theory, a citizen forms expectations about ρ and n and is more likely to vote if P is high. In most analyses, N is substituted for the citizen's expected n. The voter participation rate, $V = n/N$, is then given by:

$$V = v(P,D,X) = v[p(N,\rho), D,X] \qquad (3)$$

where D is a vector of dummy (binary) variables, and X is a vector of continuous variables not directly related to the closeness of the election. If the Downsian rational voter model is valid, one expects that, *ceteris paribus*, V rises as the strength of Republicans and Democrats in the constituency becomes more evenly balanced (i.e., as ρ approaches ½) and as the size of the electorate (N) diminishes.[6]

Extensions of the Binomial Model

In the binomial model of equation (2), P is of the order $n^{-½}$. Chamberlain and Rothschild (1981) and Margolis (1977) show that when ρ is treated as a random variable rather than as a parameter of the binomial distribution, P becomes of the order n^{-1}. The Margolis explanation is summarized here.

Let r be the number of votes for the Republican candidate, and $\rho = r/n$. A voter believes ρ is approximately normally distributed with standard deviation σ. For mathematical simplicity, let $R = (\varrho - .5)/\sigma$; R is approximately normally distributed with mean μ, unit standard deviation, and density $f(R) \approx .4 \exp[-.5(R-\mu)^2]$. Then the probability that one voter can break a tie is $P = \text{prob}(R=0) \approx f(0)dR$, where $f(0)$ is the height of the normal density at $R = 0$ and dR is the width of an interval controlled by one voter. This interval is derived as follows. For fixed n, $d\rho = (1/n)dr$, and $dR = (1/\sigma)d\varrho$. If $dr = 1$ (i.e., if one voter switches), $dR = 1/n\sigma$, and $P \approx f(0)/n\sigma$, which is of order n^{-1}.

The probability of a tie-breaking vote is logically related to the concept of voting-power indexes, especially those of Banzhaf (1968) and Shapley and Shubik (1954). Straffin (1977) defines the absolute or non-normalized Banzhaf index as the number of coalitions of n voters that could result in a tie vote divided by the total number of possible coalitions. The numerator is $n!/[(n/2)!]^2 \approx 2^{n+1}/\sqrt{2\pi n}$ (by Stirling's formula). Since the denominator is 2^n, the index is approximately $2/\sqrt{2\pi n}$, which is of order $n^{-½}$.[7] The Shapley-Shubik power index is presented as the number of ordered sequences of $n+1$ voters in which a given voter is pivotal divided by the total number of possible sequences (Straffin, 1977). For voters of equal political weight, this equals $n!/(n+1)! = 1/n$, which is trivially of the same order as the Chamberlain-Rothschild-Margolis models of P.[8]

Previous Tests of the Closeness Hypothesis

The Barzel-Silberberg Model

Barzel and Silberberg (1973) conducted one of the earliest and most often-cited statistical tests of the importance of election closeness on the rate of voter participation. Their model can be expressed as $V = v[p(n, \rho, \sigma), D]$, where σ is the standard deviation of ρ and D is a vector of dummy variables. The relevance of N and ρ was explained above. Barzel and Silberberg justify the inclusion of σ in the following way. If $E(\rho)$ is close to ½, then large σ reduces the probability that this election will be close, and V falls. But if $E(\rho)$ is not close to ½, then the larger σ, the greater the likelihood of this election being close, and V rises. They state: "In other words, we view [σ] as a potentially relevant variable, but we do not formulate a testable hypothesis regarding it" (1973, p. 53).

Using a sample of 122 pooled observations gathered at the state level for the 1962, 1964, 1966, and 1968 gubernatorial elections they obtain the following Ordinary Least Squares (OLS) regression results:[9]

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[6] Although this may accord with political intuition, one should not rely too literally upon the mathematics of equations (1) and (2). The value of n must be large for Stirling's Formula to apply, but $V \doteq n/N$ must be quite small for the actual voting process to approach the "n independent Bernoulli trials" implicit in the construction of a binomial random variable. The hypergeometric distribution is more appropriate for modeling a voting decision process where the "sampling" is done without replacement (Foster, 1983).

[7] This is the binomial probability of a tie vote when the electorate is evenly divided in its preferences for the Republican or Democratic candidate, as can easily be seen by substituting $\rho = ½$ into equation (2). This assumption of an equally divided electorate is one weakness of the Banzhaf index in explaining voter turnout (Margolis, 1977, p. 138). A variant of the Banzhaf index is employed in the Kau-Rubin model discussed below.

[8] Some studies of voting behavior have used $1/n$ as a measure of the probable closeness of the election. See Thompson (1982, pp. 307-309).

[9] Gubernatorial election data were chosen, in part, because "the issues were not clouded by electoral col-

$$V = -\underset{(7.24)}{.768\,WIN} - \underset{(1.41)}{.006\,N} - \underset{(2.41)}{.291\,DDIFF} + \underset{(5.75)}{.111\,PRES} + \underset{(2.62)}{.055\,SEN} \tag{4}$$

($\bar{R}^2 = .472$; $|t\text{-ratios}|$ are in parentheses; no constant term estimate was recorded.) *WIN,* the number of votes for the winner divided by *N,* is a proxy for ρ, and *DDIFF,* the difference in the proportion of votes for the Democratic candidate between the current and previous elections (absolute value) is a proxy for σ. *PRES* and *SEN* are dummy variables which equal 1 if a presidential or senatorial election coincided with the contest for the governorship, or 0 if it did not.

The larger *WIN,* the more ϱ departs from ½, and rational voter theory would predict that, if closeness is important, $\delta V/\delta WIN < 0$ and $\delta V/\delta N < 0$.[10] These are the signs obtained in equation (4), although *N* is not significantly negative at the 5% level. Barzel and Silberberg summarize their results this way:

> Wealth-maximizing behavior can explain, in part, why people bother to vote. The individual's incentive to vote increases the larger his estimate of the benefits to be derived from his candidate being elected, the smaller his cost of voting, and the larger the chance he perceives of his being the decisive vote. This last item is the crucial element in our analysis. . . . The strong confirmation of this last hypothesis is also a refutation of the hypothesis that individuals vote solely to meet their civic duty (1973, p. 58).

The Silberman-Durden and Crain-Deaton Models

The Barzel-Silberberg regression specification was expanded in minor ways by Silberman and Durden (1975). Using sample data from the over 400 congressional districts, they obtained the following OLS regression results for the 1962 general elections:

$$\begin{aligned} V = .265 &- \underset{(5.41)\ (10.4)}{.195\,WIN} - \underset{(3.33)}{.00003\,N} \\ &- \underset{(3.91)}{.831\,FEM} + \underset{(5.81)}{1.64\,A65} + \underset{(6.88)}{.018\,UNEMP} \\ &+ \underset{(12.4)}{.0001\,Y} - \underset{(6.87)}{.003\,BLACK} + \underset{(1.38)}{.013\,GOV} \\ &+ \underset{(4.42)}{.045\,SEN} \end{aligned} \tag{5}$$

($R^2 = .74$; $|t\text{-ratios}|$ are in parentheses.) Within each district, *FEM* is the proportion of female voters, *BLACK* is the proportion of black residents, A65 is the proportion of citizens over 65 years of age, *Y* is the median income, and *UNEMP* is the unemployment rate. *GOV* and *SEN* are gubernatorial and senatorial election dummies defined in the obvious way, and *WIN* and *N* are the same as in equation (4). Like Barzel and Silberberg, Silberman and Durden found that closeness counts—*WIN* and *N* are significantly negative.[11] They conclude: "The act of voting as a 'civic duty' is not the only determinant of voter participation" (1975, p. 107).

Crain and Deaton (1977) did not propose to test the closeness hypothesis, but in the course of pursuing other research objectives they obtained the following OLS regression results, using 50 state-level observations for the 1972 presidential election:

$$\begin{aligned} V = -2.58 &- \underset{(2.90)}{.22\,LnWIN} - \underset{(1.43)}{.02\,LnN} \\ &+ \underset{(4.38)}{1.26\,LnFEM} + \underset{(1.92)}{.08\,LnA65} \\ &+ \underset{(0.72)}{.02\,LnUNEMP} + \underset{(4.89)}{.42\,Ln(PY/N)} \\ &- \underset{(6.18)}{.05\,LnBLACK} - \underset{(0.10)}{.002\,DUM} \end{aligned} \tag{6}$$

($R^2 = .80$; $|t\text{-ratios}|$ are in parentheses.) Ln indicates natural logarithm, and *PY/N* is per-capita nominal income. *DUM* is a dummy variable that equals 1 if a senatorial or gubernatorial election (or both) coincided with the presidential contest, or 0 if otherwise. As can be seen, Ln*WIN* and Ln*N* are significantly negative at the 1% and 10% levels, respectively, and income is significantly positive.

The Kau-Rubin Model and the Banzhaf Power Index

Kau and Rubin (1976, p. 103) examined the effects of the electoral college system on voter turn-

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lege considerations which affect presidential elections" (1973, p. 54).

[10] *WIN* is based on the actual election outcome, and *N* is substituted for expected *n*. The implicit assumptions are that, on average, voters correctly perceive *WIN* before the election, and that $E(n)$ is proportional to *N*.

[11] They also find that income carries a positive coefficient. Whether $\delta V/\delta Y$ is positive or negative has been the source of some debate in the literature. See Downs (1957), Tollison and Willett (1973), Frey (1971), Frey *et al.* (1971), Crain, Deaton, and Tollison (1978), Rubinfeld and Thomas (1980), and Verba and Nie (1972).

outs in presidential elections. They first argued that "the probability of breaking a tie will be larger with direct voting than with an electoral college and we can therefore predict that abolition of the electoral college would increase voter participation." There is a flaw in their mathematical reasoning, but they did recognize that the validity of their prediction would still depend on voter rationality.[12] This they test for the 1964 election, using a modified Barzel-Silberberg regression model. They obtain the following results:

$$\begin{aligned} V = {} & \underset{(1.49)}{.005\,WIN} + \underset{(3.16)}{.011\,(1/N)} - \underset{(0.76)}{.003\,DDIFF} \\ & + \underset{(1.92)}{.194\,B} + \underset{(0.87)}{.0001\,GOV} - \underset{(2.47)}{.0003\,SEN} \end{aligned} \tag{7}$$

(R^2 = .96; $|t$-ratios$|$ in parentheses; no constant term estimate was reported.) B is a variation of the Banzhaf index which attempts to capture the voting power of a citizen under the electoral college system.

$$B = \frac{2E}{\sqrt{2\pi N}} \tag{8}$$

where E is the number of votes that a state may cast in the electoral college.

Although *WIN* is not negative, B and $1/N$ are significantly positive, and Kau and Rubin conclude that the evidence favors the rational voter hypothesis. But the weakness of the Banzhaf index for describing voter behavior has already been noted. In addition, the number of electoral votes in the numerator of equation (8) is likely to be almost proportional to N. If $E = kN$, then B is of order $N^{1/2}$, and one would expect $\delta V/\delta B$ to be negative by the reasoning above. However, Kau and Rubin divided their regression equation by N in order to reduce suspected problems of heteroscedasticity, before obtaining the estimates in equation (7).[13] This makes the transformed B of order $N^{-1/2}$, which should then carry a positive coefficient.

The Cebula-Murphy Approach to Probable Election Closeness

In the models above, the variables directly related to the rational voter hypothesis are *WIN, N, DDIFF,* and *B.* A potential weakness of *WIN* is that it is based on the *ex post* election outcome. Cebula and Murphy (1980) used an *ex ante* measure of expected closeness in a study similar to that of Kau and Rubin. They obtained the following regression results:

$$V = 93.7 - \underset{(2.64)}{23.7\,L} \tag{9}$$

(R^2 = .175; $|t$-ratio$|$ in parentheses.) L is a legislative-imbalance variable equal to the proportion of Democrats in the lower house of the state legislature. Equation (9) was estimated for the 1976 election using a sample of 35 states for which $L > 1/2$. Since L may be interpreted as a proxy for $1 - \rho$, the negative coefficient is consistent with the rational voter hypothesis. Although equation (9) is clearly incomplete as a model of voting behavior, the use of *ex ante* measures of expected closeness are preferred to *ex post* measures on theoretical grounds, and the *ex ante* model developed below is based on this approach.

Empirical Tests and Results

In all of the regression models reviewed thus far, at least one of the closeness variables (*WIN, DDIFF, B, L,* and *N*) came up with a statistically significant coefficient of the correct sign. One would conclude that previous statistical evidence is consistent with the notion that citizens evaluate P and vote more heavily in districts where the perceived P is higher than average. The following discussion examines these and other models to see how much the rational voter hypothesis contributes to the explanation of voter turnout at the state level in recent presidential elections.[14]

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[12] The flaw is corrected in Kau and Rubin (1977), where they show that, in some circumstances, the probability of breaking a tie may be enhanced by the electoral college system, as opposed to direct election schemes.

[13] (1976, Table 1.) Dividing by N transforms OLS into Weighted Least Squares (WLS). If N were completely dropped from the Barzel-Silberberg specification of equation (4), then the $1/N$ term in equation (7) may be the constant term after the WLS transformation. Kau and Rubin do not report whether they tested for the presence of heteroscedasticity. Few of the models tested in the next section display much evidence of nonconstant variance of the error term, and when they do, the transformation used by Kau and Rubin would not be appropriate.

[14] In this context, the rational voter hypothesis implicitly assumes that, because of the electoral college system and its winner-take-all or unit rule, it is appropriate to measure the expected closeness of the state, rather than national, election outcome. This is further substantiated by Aldrich, who found that "the statewide P term is related (if weakly) to voter turnout, whereas the nationwide P term is . . . totally unrelated" (1976, p. 729).

Data Base

All variables, apart from dummies and logarithms, have been scaled to have means on the order of 10 or 100. There are 200 observations on each relevant variable, arranged in four cross-sections by state for the years 1968, 1972, 1976, and 1980 (or the closest year for which the required data are available). *V* is the voter participation rate, defined as the proportion of age-eligible voters casting ballots for presidential electors. *N* is the size of the electorate, defined as the number of persons of voting age. (Both *V* and *N* could be defined with reference to registered voters, but up to eight states which do not report or require registration would have to be dropped from the sample in some years.) Median four-person family income (*Y*) is deflated by the Consumer Price Index (1967 = 1.00) to make observations comparable across years. An alternative income variable, *POV*, is the percentage of the state population living below the established poverty line. The Banzhaf index is calculated from equation (8).[15] *BLACK* is the number of black citizens aged 18 years or older, divided by the number of eligible voters in the state (*N*). A18 (voters aged 18-24), A65 (voters aged 65 +), and *UNION* (union members) are similarly constructed. *MET* is the percentage of the state population living in Standard Metropolitan Statistical Areas. *WNS* is the proportion of the state population that declared itself white and of non-Spanish origin on the decennial census.[16] Variable *Q* is discussed with the *ex ante* model. The remaining continuous variables have been defined previously.[17]

Data Pooling

Each model analyzed below was tested to see if the four cross-sections of data could be pooled and one regression run on all 200 observations. The testing procedure, explained in Maddala (1977, sec. 14-2), involves two Chow tests for parameter stability. The first tests the null hypothesis (H_0) that all regression coefficients (including the constant term) and the variance of the regression error term (σ_e^2) are stable for all four election years. If H_0 is accepted, one can pool the 200 observations and run one OLS regression without further modification to the model. H_0 was rejected for all the models in this article, and no simple pooling is used.

The second Chow test involves the null hypothesis that all parameters except the constant term are stable over the four election years. If this is accepted, the data can be pooled, a dummy constant created for each different year, and one Least Squares Dummy Variable (*LSDV*) regression can be run. *LSDV* regressions were found to be appropriate for all but the Barzel-Silberberg and Kau-Rubin models.[18]

Regression Results for Previous Models

Table 1 records the OLS regression coefficient estimates for the Barzel-Silberberg and Kau-Rubin models. The results will be in support of the rational voter hypothesis if the coefficients of *WIN* and *N* are significantly negative, the coefficient of *B* is significantly positive, and the coefficient of *DDIFF* is significantly different from zero.[19]

The actual results are very mixed. For both models, *WIN* is significantly positive at the 1% level in 1968, significantly negative in 1972, and insignificant in 1976 and 1980. *N* varies in sign and is never significant. *DDIFF* is significant only for 1976. The Banzhaf index in the Kau-Rubin model is positive twice, negative twice, and never significant.[20] Neither model appears to explain voter turnout with any degree of reliability.

Both the Silberman-Durden and Crain-Deaton models were estimated with the LSDV method.

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[15] In Banzhaf (1968) and Kau and Rubin (1976), the index number for each state is divided by the lowest *B* value (that of Washington, D.C.).

[16] People of Spanish origin may be of any race, and the category includes persons of Mexican, Puerto Rican, Cuban, and Central and South American descent. Data on the proportion of persons of Spanish origin who declare themselves as white are required to construct *WNS*. These data were found only for 1970 and were used to calculate *WNS* for all four election years.

[17] Data sources for construction of variables: *1970 Census of Population (WNS); 1980 Census of Population and Housing (POV, WNS, Y); Presidential Elections Since 1789 (DDIFF, E, WIN); Statistical Abstract of the United States (Y, WNS,* and all other variables).

[18] A third Chow test examines the null hypothesis that, given stable slope coefficients, the constant is also stable over time. All models failed this test, as expected. There are many other models which pool time-series and cross-section data, but LSDV is the most feasible when, as here, the time series is only of length 4.

[19] Significance is determined at the 5% level unless otherwise noted. One-tailed *t*-tests are used when theory predicts a sign for the coefficient a priori.

[20] It is undoubtedly because of this instability that these models failed the pooling tests. If the models are estimated with the LSDV specification anyway, *WIN* and *B* are always insignificant, *N* varies, and *DDIFF* is always significant.

Table 1. Barzel-Silberberg and Kau-Rubin Regression Coefficients[a]

	1968		1972		1976		1980	
				Barzel-Silberberg				
Constant	38.993	(2.83)	108.79	(10.5)	63.239	(5.67)	54.256	(6.21)
WIN	0.4595	(2.48)	−0.836	(5.45)	−0.013	(0.06)	0.0027	(0.02)
N	0.0241	(0.53)	−0.054	(1.60)	−0.040	(1.57)	−0.043	(1.41)
DDIFF	−0.167	(0.53)	0.2073	(0.98)	−0.482	(5.07)	0.0942	(0.30)
GOV	5.3159	(2.40)	3.6180	(1.90)	3.7416	(2.13)	1.7121	(0.75)
SEN	0.1596	(0.07)	−1.503	(0.71)	0.3205	(0.19)	−0.952	(0.47)
$\bar{R}^2$	.1799		.4213		.4366		−.0129	
F(5,44)	3.150		8.133		8.594		.8750	
				Kau-Rubin				
Constant	33.155	(2.03)	112.01	(7.89)	70.772	(5.20)	50.615	(4.27)
WIN	0.4562	(2.44)	−0.837	(5.40)	0.0031	(0.01)	−0.001	(0.01)
N	−0.048	(0.41)	−0.017	(0.15)	0.0447	(0.49)	−0.079	(0.94)
DDIFF	−0.190	(0.60)	0.2109	(0.98)	−0.506	(5.15)	0.1271	(0.39)
B	0.0145	(0.68)	−0.008	(0.33)	−0.021	(0.97)	0.0099	(0.46)
GOV	5.2119	(2.34)	3.6282	(1.89)	3.8130	(2.17)	1.7052	(0.74)
SEN	0.2205	(0.10)	3.6282	(1.89)	−0.095	(0.05	−0.953	(0.46)
$\bar{R}^2$	.1698		.4093		.4358		−.0314	
F(6,43)	2.670		6.660		7.308		0.752	

[a]Sample size is 50; | *t*-ratios | in parentheses; $\bar{R}^2$ is corrected R^2; *F*(*a*,*b*) is *F*-statistic (degrees of freedom).

The results are in Table 2. It can be seen that *WIN* and *N* (or their logarithms) have negative coefficients, but *WIN* and Ln*WIN* are insignificant. In the estimates of both models for individual election years (not tabulated), only Ln*WIN* is significantly negative, and only in 1972; the closeness variables are often of the wrong sign in the remaining years.

The other variables do better. *BLACK* is significantly negative, and *Y* and *A65* are significantly positive; all are important in terms of their elasticities at the mean (− .07, .19, and .30 in the Silberman-Durden model). This is consistent with the findings of Verba and Nie (1972) that political participation was lower among blacks and lower income groups and higher for the elderly after the

Table 2. Silberman-Durden and Crain-Deaton Regression Coefficients[a]

	Silberman-Durden		Crain-Deaton	
Constant	29.850	(3.65)	−74.00	(3.76)
YR76	2.5581	(2.40)	3.8796	(3.85)
YR72	3.4619	(2.83)	5.1539	(4.36)
YR68	8.8187	(6.55)	13.621	(10.0)
WIN[b]	−0.067	(1.05)	−2.693	(0.79)
N[b]	−0.025	(1.76)	−1.409	(2.69)
A65[b]	1.1257	(6.59)	22.659	(11.7)
Y[b]	1.0875	(2.44)	31.299	(7.88)
BLACK[b]	−0.486	(8.78)	−1.318	(6.27)
UNEMP[b]	0.9207	(2.31)	6.5929	(4.64)
GOV	2.2099	(2.65)	1.7885	(2.24)
SEN	−0.842	(1.07)	−0.702	(0.95)
$\bar{R}^2$	.5954		.6359	
F(11,188)	27.625		32.598	

[a]Sample size is 200; | *t*-ratios | in parentheses; $\bar{R}^2$ is corrected R^2; *F*(*a*,*b*) is *F*-statistic (degrees of freedom).
[b]Natural logarithm in Crain-Deaton model.

effects of income and education had been accounted for. *UNEMP* is also significant and positive.[21] Variable *FEM* in equations (5) and (6) was dropped because it showed inadequate variation across states.

Finally, note that the dummy variables *YR68, YR72,* and *YR76* are positive, significant, and smaller for more recent elections, which reflects the trend toward lower overall voting rates prevalent in the United States since 1960, but also implies that the causes of this decline are not completely captured by the continuous variables in the model.

The Ex Ante Model

Wolfram and Foster (1981) developed a rational voter model in which the expected closeness of the election was measured in a manner similar to the *ex ante* approach used in Cebula and Murphy (1980). If *DL* is the number of Democrats in both houses of the state legislature before an election and *RL* is the number of Republicans, then let

$$Q = |[DL/(DL+RL)] - ½|. \qquad (10)$$

If the political imbalance in the legislature approximates the imbalance in voter preferences for the two parties, then Q may be interpreted as a proxy for $|\rho - .5|$. The smaller Q is, the closer the election is expected to be, and according to rational voter theory, $\delta V/\delta Q < 0$.[22]

The *ex ante* model developed here combines closeness variables Q and N with vectors of other variables (X and D) similar to those chosen by Silberman and Durden.[23] *WNS* and *POV* replace *BLACK* and Y because, although they measure the same effects, the former set proved to have greater explanatory power than the latter. *UNION, MET,* and *A18* were also felt to be relevant to explaining turnout, although not directly related to rational voter theory. The LSDV regression results are presented in Table 3. One version of the model includes dummy variable S equal to 1 if the observation is for a southern state or 0 otherwise.

For the model estimates with S omitted, Q and N are significantly negative at the 1% and 5% levels, respectively, although the elasticities at the mean (−.04 and −.01) are small. For individual election years (not tabulated), the coefficient of Q is always negative but only significant for 1976 and 1972. N is always negative but never significant at even the 10% level. In all the regressions, the age, income, and ethnic variables are significant, but *UNION, UNEMP,* and *MET* vary. The coefficients of the *YR* dummy variables decline in value for more recent elections, as expected, with the difference between 1976 and 1980 being negligible. *GOV* and *SEN* contribute little to the overall explanatory power of the regressions, but are included to make the *ex ante* model more nearly comparable with the models previously investigated.

At first glance, these results seem to provide considerable support for the Downsian rational voter closeness hypothesis. The relationship is significant and relatively stable (if weak) across elections.[24] Unfortunately, Q may be a very poor proxy for the expected closeness of the election outcome. The statistical link between Q and V is dominated by the Southern Effect. Legislative imbalance has historically been higher and voter turnouts lower for southern states than for the rest of the nation. Kim, Petrocik, and Enokson (1975, p. 108) cite Key (1950) on this matter:

> V. O. Key, Jr., in his *Southern Politics,* pointed out that although voter turnout was very low in the general election in the South, turnout for the primaries was considerably higher, often approaching the norm for general elections in the rest of the country. The reason for this, he argued, was that the effective election was the primary, where competition was strong, while the general election amounted to little more than a publicly required ratification of a previous decision.

To analyze this issue, the *ex ante* model was reestimated with a southern state dummy variable, S, included. S is strongly correlated (positively or

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[21] For a discussion of the effects of economic conditions on voting patterns, see Fair (1968) and Crain, Deaton, and Tollison (1978).

[22] Another "expected imbalance" variable like Q, but using the actual vote shares of the two major parties as a proxy for expected vote shares proved to be insignificant in preliminary regressions. A more promising approach is to use immediate preelection opinion polls, but the author was unable to find the requisite data by state for all four election years.

[23] The X variables may be thought of as measuring, in part, the importance of the instrumental voting hypothesis in explaining voter turnout. Even if they are not given this interpretation, they must be included in a linear regression model if the closeness variables are to be evaluated under conditions approaching *ceteris paribus.]*

[24] Q and N are never very important, as measured by their coefficient elasticities. The insignificance of N is due in part to collinearity with *MET* and may also be influenced by the omission of a relevant variable (detailed below).

Table 3. Ex Ante Model Regression Coefficients[a]

	Without Southern Effect		With Southern Effect	
Constant	6.2555	(0.70)	8.9540	(1.09)
YR76	−1.195	(1.19)	−0.776	(0.83)
YR72	3.0380	(2.64)	1.7629	(1.63)
YR68	5.1041	(3.43)	3.9337	(2.84)
S	–		−7.272	(5.82)
Q	−0.125	(4.03)	−0.060	(1.97)
N	−0.030	(2.04)	−0.018	(1.35)
A65	1.2998	(6.45)	1.0085	(5.25)
A18	1.1551	(4.17)	0.8907	(3.43)
POV	−0.615	(4.54)	−0.068	(0.44)
WNS	0.1621	(4.16)	0.1991	(5.47)
UNEMP	0.9811	(2.51)	0.5984	(1.63)
UNION	0.1818	(1.83)	0.1259	(1.37)
MET	−0.036	(1.81)	−0.016	(0.84)
GOV	1.0461	(1.33)	1.2575	(1.74)
SEN	−0.703	(0.99)	−1.060	(1.61)
$\bar{R}^2$	.6713		.7209	
F-Statistic	*F*(14,185) = 30.030		*F*(15,184) = 35.272	

[a]Sample size is 200; | *t*-ratios | in parentheses; $\bar{R}^2$ is corrected R^2; *F*(*a*,*b*) is *F*-statistic (degrees of freedom).

negatively) with *V* and *Q* (as well as with *UNION* and *POV*). From Table 3 it is evident that including *S* increases $\bar{R}^2$, robs *POV* of significance, and reduces the magnitudes of the coefficients (and of the elasticities at the mean) of *Q* and *N*. *Q* remains significant at the 5% level, and *N* at the 10% level. The southern effect is more telling in the individual-year regressions, where *Q* remains significantly negative only for 1972 (at 1%), and *N* is never significant. If *S* replaces *Q* and *N*, the $\bar{R}^2$ increases in every year, compared with regressions estimated with *S* excluded entirely.[25]

The proper interpretation of these results is ambiguous. On the one hand, *V* may be low in the South because the general election outcome is (or has been) a foregone conclusion, so *P* is low, *Q* is high, and voters rationally stay away from the polls. On the other hand, *V* may be low in the South for cultural and historical reasons completely unrelated to the rational voter hypothesis, but *Q*, owing to its correlation with *S*, is partly capturing this effect. Seeing that *S* by itself explains more of the variation in voter turnout than *Q* and *N* together (which suggests application of the principle of Occam's Razor or "parsimonious parametrization"), combined with the observation that the solid Democratic South hasn't been solidly Democratic since at least 1964, the latter interpretation is chosen here: *Q* acts more as a proxy for *S* than for $|\rho - .5|$.

Comments on Method

Multicollinearity

When two or more right-hand side (RHS) variables are linearly related, their coefficient *t*-ratios will be biased toward zero, and they may spuriously appear insignificant. As a general rule, this problem is suspected if the correlation (*r*) between two RHS variables is higher than the correlation of either of them with the dependent variable. All of the models estimated are afflicted with multicollinearity to some degree. Given the focus of this article, linear relationships among the *X* variables are of little concern (although the $\bar{R}^2$ statistics can often be raised if one or more of the *X* variables are excluded). Collinearity involving the closeness variables is more troublesome.

For the Barzel-Silberberg, Kau-Rubin, and Silberman-Durden models, examination of the pairwise correlation coefficients (calculated from the 200 pooled observations) demonstrates high correlations between *WIN* and *DDIFF* ($r = -.31$) and between *B* and *N* ($r = .89$). The relation between *B* and *N* is not a statistical problem, but rather the result of the nature of the Banzhaf index. If *DDIFF* is dropped from the Barzel-Silberberg model, the remaining coefficients and *t*-ratios remain nearly the same as in Table 1. Multicollinearity does not account for the weakness of the rational voter hypothesis in these models.

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[25] This finding accords with those of Kim, Petrocik, and Enokson. Using factor analysis, they discovered that "differences between the South and non-South alone account for just as much variation in the (residual) voter turnout as both electoral competitiveness and legal structure do" (1975, p. 123).

Using the pooled data again, one can discern problematic correlations in the *ex ante* model between *N* and *A18* ($r = -.24$), *N* and *UNION* ($r = .32$), and *N* and *MET* ($r = .55$). If *A18, MET,* and *UNION* are jointly omitted from the model with *S* included, the coefficient of *N* increases in magnitude but is significant only in the LSDV regression (at 1%), and for 1976 (at 10%). This result is not substantially altered if only *MET* is omitted. It is not clear whether *MET* was measuring the closeness effect of *N,* or *N* was capturing voting habits associated with urban and suburban populations. In any case, this set of multicollinear relationships does not explain the weakness of *Q* when *S* is included, and the verdict on the *ex ante* model stands.

Heteroscedasticity

As noted above, Kau and Rubin (1976) assumed that the error term (*e*) in their model was heteroscedastic, with variance proportional to N^2; they divided their regression equation by *N* to correct for this. For all the models in this article, Goldfeld-Quandt tests (described in Pindyck & Rubinfeld, 1981, sec. 6.1.2) were used to test the null hypothesis that $Var(e_i) \neq f(N_i)$. If the null hypothesis is rejected, multiplying or dividing through by some power of *N* may be a suitable *WLS* transformation needed to achieve homoscedasticity.

The tests on the Crain-Deaton, Silberman-Durden, and *ex ante* models were conducted using the pooled data set. Contrary to the Kau-Rubin assumption, the evidence points (weakly) to an inverse relationship between *N* and *Var*(*e*). A possible explanation of this phenomenon is that *V* and many RHS variables are ratios, with *N* in the denominator. A regression involving just raw magnitudes (the numerators) might show an error variance directly related to N^a, where $0 < a < 2$. The "quasi-division by *N*" in the actual regressions overcompensates for this (apparently mild) heteroscedasticity in the raw magnitude relation. Experimental WLS regressions, in which the Silberman-Durden and *ex ante* models were multiplied by $\sqrt{N}$, failed to improve matters, however. The R^2 statistics increased, but coefficient estimates were dramatically changed and virtually impossible to interpret. Because of this and the fact that the Barzel-Silberberg and Kau-Rubin models showed no significant evidence of heteroscedasticity of any kind, WLS has not been used in this study.

Logit Analysis

Most rational voter models identify factors affecting voter turnout rates (*V*), but can be recast conceptually in terms of the probability that an eligible citizen will vote. Since an individual either does or does not vote, the dependent variable is binary, and probit or logit models might be used. It is possible, following Schroeder and Sjoquist (1978), to apply the logit technique to some of the models previously examined. If V_i is a proxy estimate of the probability that a citizen in constituency *i* will vote, define $VLOGIT = \text{Ln}[V/(100-V)]$. *OLS,* with *V* replaced by *VLOGIT,* then yields the "logit" coefficient estimates.[26] In all cases where this method was tried, the results were nearly indistinguishable (in terms of coefficient significance and overall goodness of fit) from those recorded in Tables 1 through 3.

Simultaneous Equation Bias

If presidential campaign resources are rationally managed, then other things being equal, one would expect campaign resources to be allocated to states with many electoral votes (*E*) and to close states, as perceived by the candidate and staff. Brams (1978) shows that an optimal campaign strategy may distribute media and other campaign expenditures among states in proportion to $E^{3/2}$. This may have important ramifications for the models estimated in the preceding section.

A more complete model of political behavior in presidential campaigns might involve two equations. Let *M* be campaign media and promotional expenditures, and *Q* be a closeness variable, for example, $Q = |p-.5|$. Then, omitting constant terms and other *X* and *D* variables:

$$V = a_1Q + a_2N + a_3M \qquad (11)$$

$$M = b_1Q + b_2N \qquad (12)$$

By the rational voter hypothesis, a_1 and a_2 are both < 0. By rational campaign resource allocation, $b_1 < 0$ and $b_2 > 0$ (because *N* is roughly proportional to E). Presumably, $a_3 > 0$. Substituting (12) into (11), we obtain:

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[26] True logit subroutines usually use maximum likelihood estimation algorithms and compute "asymptotic" *t*-ratios and chi-square instead of *F*-test statistics. One possible advantage of the *VLOGIT* regression is that the heteroscedasticity of the error term is roughly known in advance. See Schroeder and Sjoquist (1978) or Theil (1971).

$$V = (a_1 + a_3 b_1)Q + (a_2 + a_3 b_2)N \qquad (13)$$

Equation (13) is, *mutatis mutandis,* the core of the models estimated in the preceding section. But because of the omission of relevant variable M, the coefficient of Q is negatively biased, whereas the coefficient of N is biased toward zero. Of course if a_3, b_1, and b_2 are sufficiently small in absolute value, these biases will be negligible. In any case, usable data on relevant campaign expenditures by state are difficult to obtain or even define, given nationwide network TV coverage and regional differences in advertising prices, so M was omitted from the previous models. But it is possible that Q (and *WIN*) are less powerful for explaining voter turnout, and N more powerful, than the results in Tables 1, 2, and 3 indicate.[27]

Conclusions

The rational voter or closeness hypothesis states that voter turnout will be higher when the election is expected to be close, because the probability (P) that an individual vote can decide the election will be higher. Intuition suggests that, for elections at the congressional district level or higher, P will still be too small to motivate citizens to vote. Nevertheless, beginning with Barzel and Silberberg, statistical support for the hypothesis has been reported for gubernatorial elections (at the state level), congressional elections (at the district level), and presidential elections (at the state level). Perhaps one should speak of the semi-rational voter hypothesis—voters respond rationally to perceived P, but their perceptions are greatly exaggerated!

The models used in previous tests have been re-estimated for four presidential elections at the state level. The Barzel-Silberberg and Kau-Rubin models are too unstable to allow data pooling, and the evidence for the closeness hypothesis is virtually nonexistent in all years except 1972 (when *WIN* is significantly negative). The Silberman-Durden model and its Crain-Deaton semilog variant include a vector X of socioeconomic and demographic RHS explanatory variables, and perhaps because of this they are stable enough to permit pooling of data and LSDV regression. *WIN* and N are negative, but only N is significant, and it may be measuring influences associated with highly urbanized states. Neither model supports the rational voter hypothesis in individual election years (with the possible exception of 1972).

When Q replaces *WIN,* as in the *ex ante* model, election closeness effects appear to be significant and relatively stable from year to year, although of small magnitude. But when the regional voting patterns in the South are accounted for, the closeness effect is greatly reduced in both significance and size, and virtually disappears in the 1968, 1976, and 1980 elections. Given the relationship between Q and S, and between N and a number of the X variables, the evidence in favor of the rational voter hypothesis in the *ex ante* model, although stronger than in the others, is still weak overall and unpredictable on a year-to-year basis.

The general conclusion of this article, especially in view of the simultaneous equation bias which may be present in the results, is that the perceived probability of a tied election at the state level is not a powerful or reliable factor in explaining across-state variation in voter participation rates in presidential elections.

The statistical results upon which this conclusion is founded can be used to shed some light on the matter of declining voter turnouts. The proportion of the U.S. voting age populations actually voting for president has steadily diminished from 62.8% in 1960 to 53.2% in 1980. This decline has been the source of some alarm to observers of the democratic process and has caused concern among those who believe that low voter turnouts favor the Republican party.[28]

When Q and N are excluded from the *ex ante* model and the Southern effect dummy S is included, LSDV regression results demonstrate that the three most important explanatory variables (in terms of coefficient elasticities) are *WNS* (elasticity = .29), *A65* (.27), and *A18* (.29). All three variables are positively related to V. But from 1968 to 1980, the average value of *WNS* fell steadily from 85.3 to 81.5, *A65* fell from 15.8 to 15.0, and *A18* fell (but not steadily) from 20.4 to 18.6. Thus age and ethnic factors within the model appear to have contributed to the overall drop in voting rates.[29] They cannot account for all of the decline, however, because the coefficients of the individual year dummy variables (which capture trends otherwise excluded from the model) are generally positive and ranked *YR68* > *YR72* > *YR76.*

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[27] Welch (1981) uses a simultaneous equation approach to argue that campaign *contributions* may fall when an election is perceived as "too close" by the electorate. If closeness at the state level is relevant for contributions, and if these contributions are spent for local campaign advertising, then the model in equations (11) and (12) would require further modification.

[28] But see DeNardo (1980). The decline in turnout rates may not be as alarming as it appears. Abrams and Settle (1976) argue that attempts to maximize V would not be socially optimal policy when one compares the costs of increasing electoral participation with the potential costs of nonparticipation.

[29] *UNION* is also positively related to V, but with a lower elasticity (.04), and showed a steady decline from

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The Etiology of Partisan Competition

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In this inquiry, we focus upon the question, "Why are political parties more competitive in some states than they are in others?" We have measured partisan competition as the closeness between the two major political parties in the results of state-level elections, and we have chosen to analyze elections in American states in the 1970s. To account for variations among the states on the dimension of partisan competitiveness, we formulate and present four discrete lines of reasoning—including sociodemographics, urbanization, diversity, and the strength of party organizations. We test each of these avenues of explanation in turn, always taking into account the watershed between North and South in partisan competition. The results vindicate the utility of our general approach, although we do find that each of the four modes of explanation is wanting in some respect. In a full-blown multivariate context, we report that educational levels and urbanization do influence variations among states in partisan competition. Equally compelling and perhaps more interesting, the strength and activism of local party organizations in the states profoundly affect political competitiveness, even when we take stark regional differences into consideration.

In the 1940s, V. O. Key, Jr. immersed himself in the politics of the one-party South. He sought to show in rich detail how deeply characteristic patterns of southern politics were rooted in the success of Black-Belt whites in imprinting their will upon their own states. Yet Key's profounder interest lay in that quintessence of democratic politics, competition for power. He demonstrated unmistakably the impoverishment of self-government without political parties. Bereft of organized parties to marshal and express competing viewpoints about public policies within the southern states, politicians had to engage in struggles inside the ephemeral and Gothic embrace of factions in the Democratic party. One-party factionalism, Key found after nearly four years of intensive research, fosters discontinuities of leadership, confuses voters, succours politically irresponsible behavior among elites, encourages the recruitment of self-serving, demagogic leaders, enhances the political influence of capricious interests, induces favoritism unbridled by the general standards possible with organized politics, and instigates a high degree of instability in the operations of government. Nearly in exasperation, Key concluded that "although it is the custom to belittle the contributions of American parties, their performance seems heroic alongside that of a pulverized factionalism" (Key, 1949, p. 303).

Key's seminal research on southern politics both brought into sharp relief the crucial functions that partisan competition provides for democratic politics, and initiated an enduring, although episodic, scholarly interest in competition between the political parties. The corpus of research on partisan competition is now quite large.[1] Scholars have paid much attention to calculations of the extent and scope of interparty competition in various electoral units, and some

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[1] There is a substantial literature on interparty competition. The textbook literature on the subject is well represented by Eldersveld (1982, pp. 59-61), Goodman (1980, pp. 163-165), Key (1964, pp. 282-311), Morehouse (1981, pp. 45-90), and Sorauf (1980, pp. 37-54). A very considerable and perhaps somewhat tiresome part of the relevant literature has mainly to do with taking measurements of interparty competition. The most pertinent of this literature on measurement includes: Broh and Levine (1978), Casstevens and Press (1963), David (1972a, b), Dawson and Robinson (1963), Elkins (1974), Golembiewski (1958), Gray (1976), Gross (1982), Hofferbert (1964), Meltz (1973), Miller (1974), Pfeiffer (1967), Rae and Taylor (1970), Ranney (1965, 1971, 1976), Schlesinger (1955, 1960), Stern (1972), and Tucker (1982a, b). That measures of various kinds of interparty competition at the level of the American states are very highly intercorrelated, Zody and Luttbeg (1968) demonstrate very clearly. More recently, Jacobs (1982) has used an indicator of partisan competition in a study of the sources of inequality.

have attempted to account for variations in partisan competition across jurisdictions. Nevertheless, the means for research set out first by Key remain far from realized. We can still ask, "Why are political parties more competitive in some American states than they are in others?"

The Crux of Partisan Competition

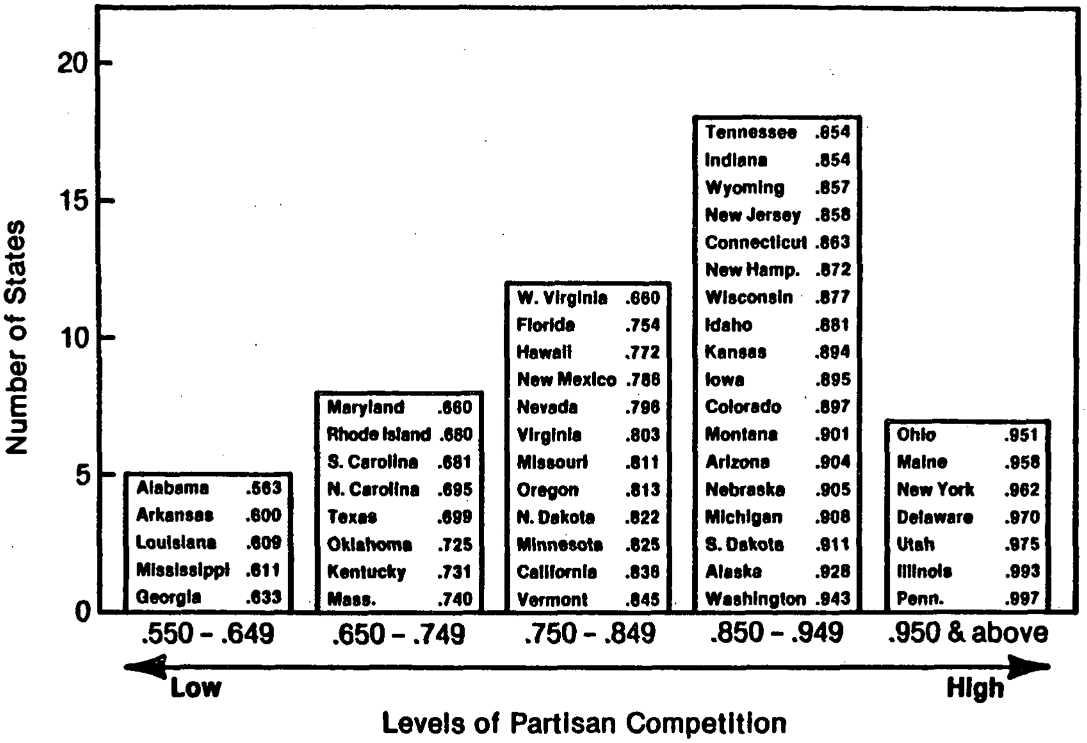
Partisan competition has come to stand for a number of rather different political phenomena. Cross-national studies dealing with partisan competition emphasize the number of political parties. Here, of course, political scientists have done investigations at the level of the party system and have drawn the customary distinctions among one-party, two-party and multiparty structures (e.g., Duverger, 1954, pp. 206-280; Epstein, 1980; Sartori, 1976, pp. 131-216). Theorists of rational choice have sought a logic of party competition in the vote-maximizing strategies of politicians (e.g., Barry, 1970, pp. 99-125, 151-153; Downs, 1957, pp. 24-30; Laver, 1981, pp. 122-146; Robertson, 1976; Schumpeter, 1950, pp. 269-283; Stokes, 1963); the number of parties constitutes the condition under which teams of leaders compete for votes, and the competitive strategy itself is the analytical focus. Yet others conceive of party competition mainly as a matter of the alternation of parties in office (e.g., Schlesinger, 1955, 1960, 1966, pp. 58-65). Such a conception of competitiveness is especially congenial in parliamentary systems where the party battle is zero-sum, so that the partisan voters can be said to have thrown the rascals out; expositions on partisan competition in Britain incline in this direction (e.g., Finer, 1980, pp. 3-31; Rose, 1980, pp. 9-32). Beyond assessing the existential phenomenon of alternation in party control, some have understood competition as the degree of uncertainty about electoral outcomes for the parties—could an election go either way? Multiparty systems present especially vexing difficulties in constructing indicators of the probabilities that the competing parties could have won (see Elkins, 1974; Stern, 1972). Finally, in the American context, scholars have commonly assessed competition in terms of the closeness of the two major parties: the proportion of close outcomes over a span of elections, the average electoral margins for selected offices, the evenness of party contingents in the legislature, the division of party control between the executive and the legislature, or margins between parties in registration, partisan identification, or participation in primaries (Jewell & Olson, 1978, pp. 19-50; 1982, pp. 17-45).

In our inquiry, we draw upon indications of the degree of closeness between political parties in contriving a measure of partisan competitiveness. Because our analytical unit is the American state, we can construct scores for partisan competition following the conventional procedures (see Bibby et al., 1983a; Dawson & Robinson, 1963; Key, 1964, pp. 282-311; Ranney, 1965, 1971, 1976; Ranney & Kendall, 1954). Over the period 1970-1980, we calculated a score for each of the 50 states by computing 1) the mean proportion of the popular vote won by Democratic candidates for governor, 2) the means of percentages of seats in the state legislative houses held by Democrats, and 3) the proportion of all terms for governor, senate, and house of representatives controlled by the Democrats. Taken together, these means produce an index of inter-party competition; a score of 0 indicates Republican success, a score of 1.0 indicates Democratic victory. To purge this indicator of partisan direction, we folded the partisan ends of the continuum together and subtracted the resulting number from 1.0. Thus, our scores on partisan competition for the states range from .5 to 1.0; a higher score indicates greater competitiveness irrespective of partisan direction.

Figure 1 shows the extent of variation among states in partisan competition over state elections held between 1970 and 1980. Levels of completion ranged from a low of .563 in Alabama to a high of .997 in Pennsylvania. The least competitive states were those of the old Confederacy, as one might easily have surmised. Despite increased competition between the parties in presidential contests, the southern states remain strongly Democratic in state contests and particularly in the memberships of their state legislatures. There were no one-party Republican states in the 1970s; even the two most Republican, North Dakota and Vermont, were moderately competitive. The highly competitive party systems (states with scores at .950 or greater) encompassed a rather heterogeneous lot, including the large industrial states of Ohio, New York, Illinois, and Pennsylvania, but also the company state (Delaware), the New England state of Maine, and the Western state of Utah. Fully half the states had competitive two-party systems in the 1970s, those with Tennessee's and Indiana's score of .854 and higher. Twenty were modified one-party states, from Maryland's score of .660 to Vermont's .845. Five states (Alabama, Arkansas, Louisiana, Mississippi, and Georgia) were classified as one-party systems.

Competition in the states has been monitored over a long enough period of time so that we can make statements about continuity and change. The basic patterns are portrayed in Table 1. Insofar as the partisan direction of competitiveness goes, it can be said that the number of solid Democratic states had dropped by about half, comparing the state of competition in the 1950s to that in the 1970s. Moreover, the number of modi-

Figure 1. Partisan Competition in the American States in the 1970s



fied one-party Republican states has declined sharply, the number of modified one-party Democratic states has increased, and the number of two-party states has not fluctuated much. This index of inter-party competition indicates the degree of aggregate change in partisan direction. Overall, the states have become more Democratic, yielding an average value of .562 in the 1950s, .585 from 1962 to 1973, and .620 in the 1970s. Some states have become significantly more competitive since the 1950s, for example, Florida, Maine, North Dakota, South Carolina, South Dakota, Tennessee, Vermont, and Virginia. A few states have become substantially less competitive, such as Hawaii, Massachusetts, Nevada, and Rhode Island. But the dominant pattern remains one of considerable stability of competitiveness.

In sum, the extent of partisan competition in the states has not changed much, witnessed by the mean partisan competition scores arrayed in the last column of Table 1. This score has hovered around .820 since the mid-1950s. It seems fair to say that the quantum of partisan competition in state elections did, indeed, expand some in the 1960s and has remained quite constant since then. Although some states have become more, and others less, competitive over the years, continuity overwhelms change decade by decade. Powerful correlations between decadal indicators for the states provide impressive evidence of the continuity of competition across time. The correlation of the states' scores for the 1950s and those of the 1960s (1956-1970) is $r = .87$; for the 1960s with the 1970s, $r = .83$.

We should, at this point, mark out the scope of this study of partisan competition. First of all, we focus upon state elections and the partisan division of the state legislatures; because these targets of competitiveness are state-specific, more or less insulated from the national influences induced by presidential or even congressional contests, they seem more appropriate to the unit of analysis, the state. The customary indicator of competition that we have calculated does somewhat overweight the partisan division of the legislature vis-à-vis the gubernatorial election, so that our measure does not fully capture the recent growth in competitiveness for the office of governor (and see Tucker, 1982a, b). Our indicator of variation in partisan competition admittedly does not reflect change within the brace of elections covered, but, as we have explained, the degree of aggregate change in state partisan competition is not very great over a period of more than a quarter of a century, and even less within the decade of the 1970s. Accordingly, we think that the measure of partisan competition for the American states constructed for elections in the 1970s makes for quite an appropriate dependent variable. Further, we are naturally prompted to ask, "Why are some states more competitive than others?"

Four Explanations of Partisan Competition

On one hand, explanations of differences among subnational units in partisan competition abound. The topic has come to support a rather fecund literature, replete with elaborate interpretations of the etiology of political competitiveness. Scholars have examined or proposed an impressive variety of causal influences. Yet, on the other hand, surprisingly enough, a fully satisfactory accounting for variations in partisan competition among the states has not been adumbrated or tested empirically (but see Gatlin, 1968). For the most part, students of the causes or correlates of partisan competition have proceeded on the basis of bivariate comparisons or, for more elaborate models, on the evidence from seriatim treatments of the several variables.

Sociodemographic Influences

Four distinct avenues toward the explanation of variations in competitiveness have emerged over the years, although they are often overlapping and criss-crossing routes. One of these is the *sociodemographic* explanation. Its clearest exemplar, Ranney (1976), underscores the role of the Civil War and the regional political cleavage it created in his account of differentials in partisan competition. Beyond the basic North-South split, he stresses socioeconomic variables of the kind commonly measured by indicators of income, education, race, foreign birth, or membership in a trade union. From his perspective, "the states' differences in interparty competition . . . are related to their differences in certain social and economic traits and in historical experiences and traditions" (Ranney, 1976, p. 65). Of course these regional and socioeconomic influences serve as markers for underlying distributions of attitudes and opinions. The modalities of distributions of crucial opinions within active publics shape the contours of the structure of partisan competition (especially see Downs, 1957, p. 114-141; Stokes, 1963).

No doubt, these so-called sociodemographic influences do not capture the complexity and richness of the crucial underlying distributions of opinion. In a related discussion, one student of democratic competition has wisely observed that "although socio-economic differences help to account for patterns of opposition, they leave a good deal unexplained" (Dahl, 1966, p. 370). Why? Because "the causal chain from one's socio-economic position to one's overt political action is long and tenuous; and each link in the chain may be weak enough to be broken by the pull of other forces" (Dahl, 1966, p. 370).

Urbanization

Along a second and well-traveled avenue toward explaining variations in partisan competition, *urbanization* has carried a great deal of the burden. The most eloquent exposition of this line of analysis is V. O. Key's. He claimed that "the cleavage between metropolitan residents and rural and small-town dwellers has become a most significant foundation for dual systems of state politics" (Key, 1956, p. 229; 1964, pp. 282-311). The anonymity of life in the metropolis, the den-

Table 1. The Incidence of Partisan Competition in the American States, 1948-1980

	Number of States				Means	
	One-party Democratic	Modified one-party Democratic	Two-party	Modified one-party Republican	Index of interparty competition	Partisan competition score
1948-1960	11	8	21	10	.562	.783
1956-1970	7	12	26	5	.604	.817
1962-1973	7	13	23	7	.585	.832
1974-1980	8	19	22	1	.643	.814
1970-1980	5	18	25	2	.620	.821

Sources: Ranney (1965, 1971, 1976) and Bibby et al. (1983).

sity of population, and the consequent possibilities of political mobilization may indicate an intrinsically urban effect upon partisan competition; accordingly, some investigators have dwelt principally upon the impact of the size of political units on competitiveness (see Black, 1974; Dahl & Tufte, 1973, pp. 89-109). But these scholars have understood that the effect of urbanization or size was not at work alone. Despite his emphasis upon urbanization as "one of the most common foundations for two-party competition," Key was well aware that "emphasis on the urban-rural foundations of party competition obscures a complex reality. . ." (Key, 1964, pp. 295, 297). For Key, this complexity revolved around differences associated with socioeconomic variables; for Dahl and Tufte, and for Black, a more diffuse heterogeneity or diversity provides an important part of the underlying foundation for competitive politics in the urban milieu. For others, the root cause of competitiveness lies not in urban environments as such, but in Americans' migratory tendencies and in urban growth, processes that both urbanize and diversify. These developments have led, Sorauf remarks, to "creeping competitiveness" (Sorauf, 1980, pp. 53-54; for a historical analysis, See Degler, 1964).

Despite the appeal of the urbanization hypothesis, the empirical evidence is very mixed.[2] Figure 1 shows that among the most competitive of American states, many are also highly urban and industrialized (viz., Ohio, New York, Illinois, and Pennsylvania), but others are substantially rural (Maine and Utah). Research on a number of rural states (Montana, Mississippi, Vermont, and Iowa) has indicated a highly inconsistent relationship between urbanization and partisan competition (Bryan, 1981, pp. 109-154; Gold & Schmidhauser, 1960). Although metropolitan environments may provide the kind of diversity that supports partisan competition, they may also foster uniparty political mobilization and depress competition (e.g., Black, 1974; Epstein, 1980, p. 53). A satisfactory explanation for variations in partisan competitiveness cannot spring from urbanism alone.

Diversity

Although the notion of diversity provides one of the main props for scholars who travel the avenues of sociodemographics or urbanization, some hands have taken it as the major route of explanation, at least potentially independent and self-composed. Elaborating James Madison's argument in *Federalist No. 10,* Rae and Taylor postulate that in large, although not necessarily urbanized, republics, "the more diverse the interests within the electorate, the more diverse the organized factions within it" (Rae & Taylor, 1970, p. 7). Some investigations have involved analyses of the direct effects of diversity on partisan competition (e.g., Fiorina, 1974, pp. 89-119; Sullivan, 1973). But such efforts run afoul of the complexity and subtleties of the development of partisan competition in particular environments. Differences in the sizes of communities, between regions, and among indicators of diversity (e.g., socioeconomic versus racial diversity), among other things, confound empirical relationships (Black, 1974, pp. 1253-1257; Bond, 1983; Fiorina, 1974, pp. 94-100; Sullivan, 1973, pp. 80-84).

Organizational Strength of Parties

In a fourth line of reasoning, scholars have focused upon the explicitly political ecology in which competitive party politics arises. Here the hermeneutic thrust goes largely to party organizational leadership, strength, activity, or intensity. Party organizations and leaders, so the argument runs, activate, aggregate, and mobilize voters of perhaps diverse backgrounds who share similar political interests, opinions, or desires. Chambers and Davis show how party organization dramatically fostered both competitiveness and electoral participation in the states as organized parties emerged beginning in the mid-1820s (Chambers & Davis, 1978). Other inquiries have indicated that, although parties may better organize themselves in the prospect of electoral success, organization and leadership seem to be preconditions for partisan competition (see Beck, 1974; Bibby et al., 1983a, pp. 76-85; Bryan, 1981, p. 111; Gatlin, 1968, pp. 239-243; Harmel & Janda, 1982, pp. 42-57).

For those states about which scholars have some knowledge of the relationship between organizational development and competition, we learn that the nexus linking the two is extremely complex.[3] Idiosyncratic features sometimes seem to take the driver's seat in observations of a single state. Frequently we observe that the exertion of leaders, or organizational activity, is especially effective for the minority party that seeks to parlay itself into a competitive role (e.g., Crotty, 1968; Katz & Eldersveld, 1961). On occasion, when a state party establishes an effective organization and a coterie of popular and competent leaders, it can achieve dominance. There is, of course, a mode of analysis in which, over the long haul, the national political parties can and do maintain a competitive equilibrium (Sellers, 1965; Stokes & Iversen, 1962). Nevertheless, at the subnational level, party dominance is often in evidence, so that organizational efforts certainly are not, by themselves, sufficient to nurture competitive party politics (see May, 1973).

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[2] Studies focusing on urbanization and party competition flourished following Key's early work. Some of these studies found urbanization to be highly related to competition, and some found the contrary to be the case. These urbanization studies differ methodologically and deal with different levels of analysis. The major studies include: Bonjean and Lineberry (1970), Bryan (1981), Coulter and Gordon (1968), Cutright (1963), Eulau (1957), Franklin (1971), Gatlin (1968), Gilbert and Clague (1962), Gold and Schmidhauser (1960), and Gordon and Coulter (1969).

[3] Some studies of the politics of individual states contain insights about the bases of partisan competition within a state and about the etiology of competitiveness.

These four well-trodden avenues toward explaining variations in partisan competition among American states do not, of course, exhaust the explanatory possibilities. It is, for instance, a commonplace to speak of the impact of national political events, personalities, or critical elections on the contours of state party competition. Such national influences as the Civil War, the New Deal, and the election of Dwight D. Eisenhower in 1952 may deeply affect the competitive capability of a state party for many years thereafter. A state's competitive structure may be interdicted by the remarkable performance of particular leaders or movements; the examples of the La Follettes and the Progressive party in Wisconsin, the impact of Edmund Muskie on the Democratic party in Maine, or the effect of Henry Belmon on the emergence of the Republican party in Oklahoma come readily to mind.

Again, partisan competitiveness in a state may be shaped by historical experience, patterns of population migration, geographical location, or the values, norms, and styles of politics that come with different political cultures (see Elazar, 1972; Jewell & Olson, 1982, pp. 5-11). Finally, states' levels of partisan competition can emanate from varying admixtures of competitiveness in their component subsystems, some procrustean and others very supportive. Furthermore, the durability of partisan competitiveness may depend heavily upon the intra-state competitive mix among subconstituencies (see Lindeen, 1971; Schlesinger, 1966, pp. 133-135; Wolfinger & Greenstein, 1969). Moreover, the capacity of a state party to survive electoral drought may depend upon its holding some critical mass of safe counties or communities, a stronghold that permits it to fight again another day (e.g., see Sutton, 1982).

Building Models of Partisan Competition

These four theoretical avenues for explaining partisan competition provide important guidance in the selection of independent variables appropriate to a multivariate analysis. Scores on partisan competition for state electoral outcomes from 1970 to 1980, nondirective as to party, provide the dependent variable. We have as our purpose to contrive a model that will give a good accounting for variance in state partisan competition, including independent variables chosen because of theoretical cogency and statistical utility. Because a congeries of specific independent variables has been proposed to have a direct bearing on variations in partisan competition, we have been called upon to screen more than a score of contenders. Some we could discard on theoretical grounds, and some provided no help in a statistical sense either by way of bivariate correlations or inclusion in a preliminary multivariate equation, or both. We intend, as the previous discussion indicates, to analyze variations in competition by using indicators of socioeconomic diversity, urbanization, the nature of the political environment, and the ubiquitous watershed of North and South.

Northern and Southern States

Of course it remains very much the case that the southern states are less politically competitive than the northern states. It is plain enough from Figure 1 that the 11 states of the old Confederacy are mighty low on competitiveness. Five yield competition scores that put them in the cellar (although all have higher scores than in the 1950s); three are in the second-lowest category (North and South Carolina, and Texas, with scores in the .650-.749 range); two are in the third-lowest groups (Florida and Virginia, in the .750-.849 range); and one, Tennessee, has achieved two-party competitive status. By contrast, the northern states have mostly high scores (but not all; Maryland, Rhode Island, and Massachusetts,

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Lockard's book on the New England states is very interesting in this regard, especially in dealing with highly competitive Connecticut (Lockard, 1959, pp. 228-304). Gosnell's research contributes to understanding the development of partisan competition in the pre-World War II era, and it stands as a remarkable example of the early use of multiple regression analysis in political research (Gosnell, 1942). Bryan's (1981) very competent recent study includes analyses of competition in Mississippi, Montana, and Vermont. For a sampling of interesting single-state studies, see Bryan (1974, pp. 68-124), Epstein (1958, pp. 33-97), Jewell and Cummingham (1968, pp. 26-178), Larew (1980), Owens, Costantini, and Weschler (1970, pp. 25-53), Pettus and Bland (1979, pp. 74-122), and Scales and Goble (1982, pp. 334-359).

for instance, are in league with some of the Old South states for low competitiveness).

In a bivariate regression in which we enter a dummy variable for region, separating the 11 Deep South states from the others, the result is a highly significant relationship statistically. This divide between North and South confounds the possibility that racial diversity could contribute to a generic explanation for variations in partisan competition. In the northern states, we encounter no significant relationship between the proportion of blacks in the population (or, conversely, the proportion of whites) and the incidence of partisan competition. But in the South, racial diversity and partisan competition are strongly correlated—as the size of the black population goes up, the level of partisan competition declines ($r = -.55$). Here we observe a pattern of party competition, or lack of it, in response to the racial composition of the southern states, very much in keeping with Key's observations nearly four decades ago (Key, 1949, especially pp. 664-675). In a multivariate model, we require region as a control for a host of differences, great and small, between North and South. Marking off the southern states will also in one fell swoop encompass the peculiar regional effect of racial concentration on party competition.

Socioeconomic Diversity

That partisan competition must derive in some way from heterogeneity in the human environment is a very plausible idea; after all, differences among people in socioeconomic status or social class commonly provide the diversity from which partisan competition can flow. Our analysis includes four indicators of socioeconomic diversity. The first of these measures the extent of educational attainment in each state, estimated as the proportion of people with a grade school education or less. A large proportion indicates a state in which many are poorly educated, and there we expect a low level of partisan competition. Figure 2 depicts the strength of this relationship very well. Other things being equal, a 1% increase in the proportion of the states' citizens without a high school education or better draws partisan competition down by one point. Competitiveness in politics, like so many other political phenomena, is highly sensitive to differences in levels of educational achievement.

Two indicators of the diversity of incomes adorn our analysis: one of poverty and one of inequality of distribution. The first, which we simply call income, consists of the proportion of households in each state whose 1980 incomes were below $10,000; the second, income inequality, is a Gini index. Both bear a modest, but nevertheless significant, bivariate correlation to partisan competition; increases in the proportion of the impoverished and increases in the inequality of the income distribution in states depress partisan competition. Figure 3 shows the general bivariate relationship between income and competitiveness. *Ceteris paribus,* a 1% increase in the proportion of households under our threshhold for poverty leads to a 1.1% decline in partisan competition. Income inequality has a similar effect.

Finally, as an indicator of the diversity of social class, we include the rate of owner-occupied housing, measured simply as the percentage of a state's dwelling units occupied by their owners in 1980. This variable has no bivariate correlation to partisan competition, but we included it in our analysis because it proved to have some explanatory value when allowed to cohabit with other independent variables.

Now we can combine these variables, forming what we might call a socioeconomic model of partisan competition. This model, formalized, is:

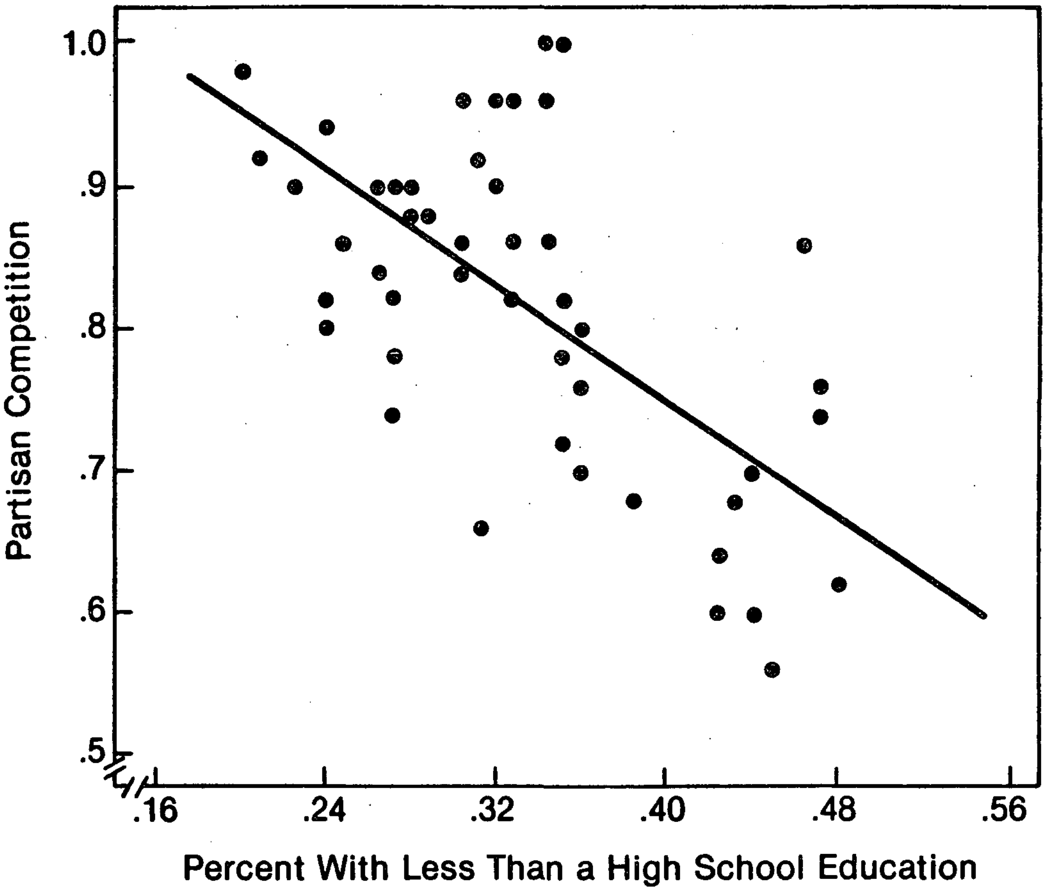
$$PC = b_0 + b_1E + b_2I + b_3IE + b_4O + b_5R + e,$$

where PC represents party competition; E, educational level; I, income; IE, income inequality; O, owner-occupancy; and R, region. We expect b_1, b_2, and b_3 to bear negative signs because of our manner of scoring these variables. Of course we include region in this model to demarcate the striking difference between North and South in the incidence of partisan competition. Estimation of this model yields the following parameters (t-ratios appear in parentheses below the coefficients):

$$\begin{aligned} PC = {} & \underset{(1.57)}{.904} - \underset{(-1.65)}{.453E} - \underset{(-.95)}{.355I} - \underset{(-.33)}{.379IE} \\ & + \underset{(1.39)}{.375O} + \underset{(2.74)}{.106R} + e. \end{aligned}$$

First of all, we find that the coefficient of determination for this equation is $R^2 = .53$, a rather considerable improvement over the statistical fit of competition on region standing alone ($R^2 = .43$). But, second, the indicators of income do not reach significance, and both educational level and owner-occupancy exert only a weak impact here. In short, socioeconomic variables by themselves have an effect on variations in partisan competition that we find somewhat disheartening given

Figure 2. Education Level and Partisan Competition



the promise of some of the existing, largely bivariate, scholarly research.[4]

We commented earlier about the use of a generalized variable for diversity in some previous work on partisan competition (especially Sullivan, 1973). We have constructed several versions of such a composite index and found it to be a futile effort. Such composites customarily combine indicators for educational attainment, income, occupation, home-ownership, ethnicity, and religion. If we omit race, the construct has a questionable conceptual status; if we include race, the index becomes difficult to understand, since race has quite different effects from those of other elements of the composite score (for an instance of the dire consequences, see Bond, 1983). A composite score for diversity made no sense in our analysis, partly for the reasons already indicated and partly because components of the diversity score varied wildly in the direction and sign of their individual correlations to partisan competition. We quickly concluded that our investigation would require us to analyze each of our socioeconomic indicators of diversity separately. Of the variables often counted in a composite index of diversity, two—ethnicity (foreign or native born) and religious affiliation—did not muster a significant bivariate correlation with partisan competition and could not fruitfully enter into a multivariate formulation.

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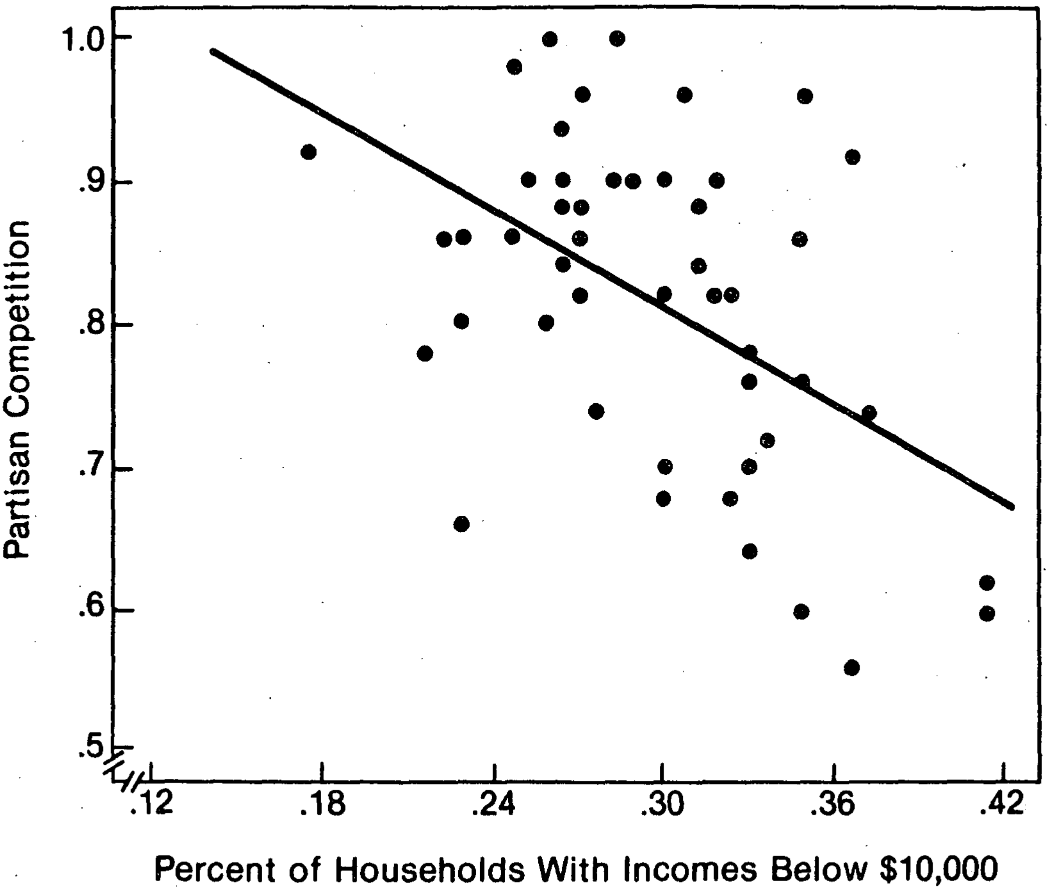
[4] We might have portrayed socioeconomic diversity somewhat more faithfully had we entered first-order polynomials for education, income, income inequality, and owner-occupancy. We have estimated the equations with two entries for each socioeconomic variable—its raw value, and its value squared—to fit the curvilinear expectation which diversity might anticipate. Doing this adds nothing to the statistical analysis and renders the coefficients difficult to interpret directly, so we have abjured the temptation to rest on this more complex model. Moreover, we have calculated the full model, which includes all independent variables, using quadratic terms for the socioeconomic variables. Doing this enhances the model's fit somewhat (from $R^2 = .68$ to $R^2 = .71$); the effects of all of the independent variables except party organizational strength decline in potency, including the effect of education, and party organizational strength retains a highly significant parameter estimate. Again, we refrain from including these quadratic terms in the full model for the sake of preserving the straightforward interpretation of our estimates, considering that, by including them, our conclusions would not be shaken.

Urbanization

In the raw, indicators of urban-versus-rural contexts do not seem to hold much promise in an explanation of competition, at least for these data from 1970 to 1980. Reacting to a body of claims about the effects of urban life on politics, we created six measures of urbanization: percentage urban population, i.e., living in towns of 2,500 or larger; metropolitan population, i.e., residing in Standard Metropolitan Statistical Areas, so-called SMSAs; percentage living in the largest metropolis; the size of the state's population in 1980; the density of population per square mile; and the extent of population growth between 1970 and 1980. None of these variables manifested a significant zero-order relationship to partisan competition. Nevertheless, it seemed to us that Key's insight was correct that the division of a state's population between major metropolises on one hand and small cities, towns, and countryside, on the other, established the ecological niche in which partisan competition might best thrive. Moreover, we thought that the mere magnitude of states' populations would, in itself, permit the kind of diversity needed to foster competitive political parties.

Figure 3. Income Levels and Partisan Competition



Accordingly, we constructed an urbanization model of partisan competition which includes both the proportion of the states' populations residing in metropolitan areas and the aggregate size of the states' populations (in millions of people):

$$PC = b_0 + b_6M + b_7P + b_5R + e,$$

where M stands for the percent of the states' populations residing in SMSAs in 1980 and P for the total 1980 population of the states; and other terms remain as before. We anticipated that b_6, b_7, and $b_5 > 0$. The estimates of this model, as we have already hinted, did not achieve the promise we had expected from our reading of the literature. Here we show these estimated coefficients:

$$\underset{(16.44)}{PC = .683} - \underset{(-.85)}{.056M} + \underset{(1.83)}{.003P} + \underset{(6.33)}{.187R} + e.$$

The R^2 for this equation is .47, so that the indicators of urbanization add only marginally to the explanatory usefulness of region alone. Even worse, metropolitanization does not reach statistical significance, although the size of population does ($p < .05$). Nevertheless, on theoretical grounds, if on no other, we persisted in believing that we should carry these measures of urbanization, the most fructiferous we could contrive, over to the more embracing multivariate model.

Organizational Strength of Parties

For a measure of the strength of the organization of state political parties, we have relied upon meticulous research, conducted during 1979-1980, designed to score the county party organizations in the states on the dimensions of organizational structure and programmatic activity (the indicator we have built from Gibson et al., 1982, Table 8).[5] In our combined scale we give a high score to states in which both Democratic and Republican county-level parties rank relatively high in organizational strength, and, conversely, a low one to states in which both rank relatively far down in strength.[6] We contend that states in which the local, grass-roots party organization is reasonably well developed across the board should provide more congenial ecologies for the emergence of partisan competition than otherwise. In addition, we take aggregated county-level scores for party organizational strength as especially desirable ones, since they are in all probability largely free of the contamination of anticipatory competitiveness of races for statewide office. Organizational activity at the state level is, without doubt, very much a function of the competitive situation for statewide office; local organizations, although influenced in some part by the fate of the state party, respond in greater measure to the particular circumstances of their immediate environment.[7]

Our measure of party organizational strength arrays the states on a scale from 1 to 50. The relationship between party strength and competition is a strong one, as Figure 4 very clearly shows. A one-point increase on the party strength scale brings about an approximately .04 increase in partisan competition, other things being equal. This conception—that partisan competition is a func-

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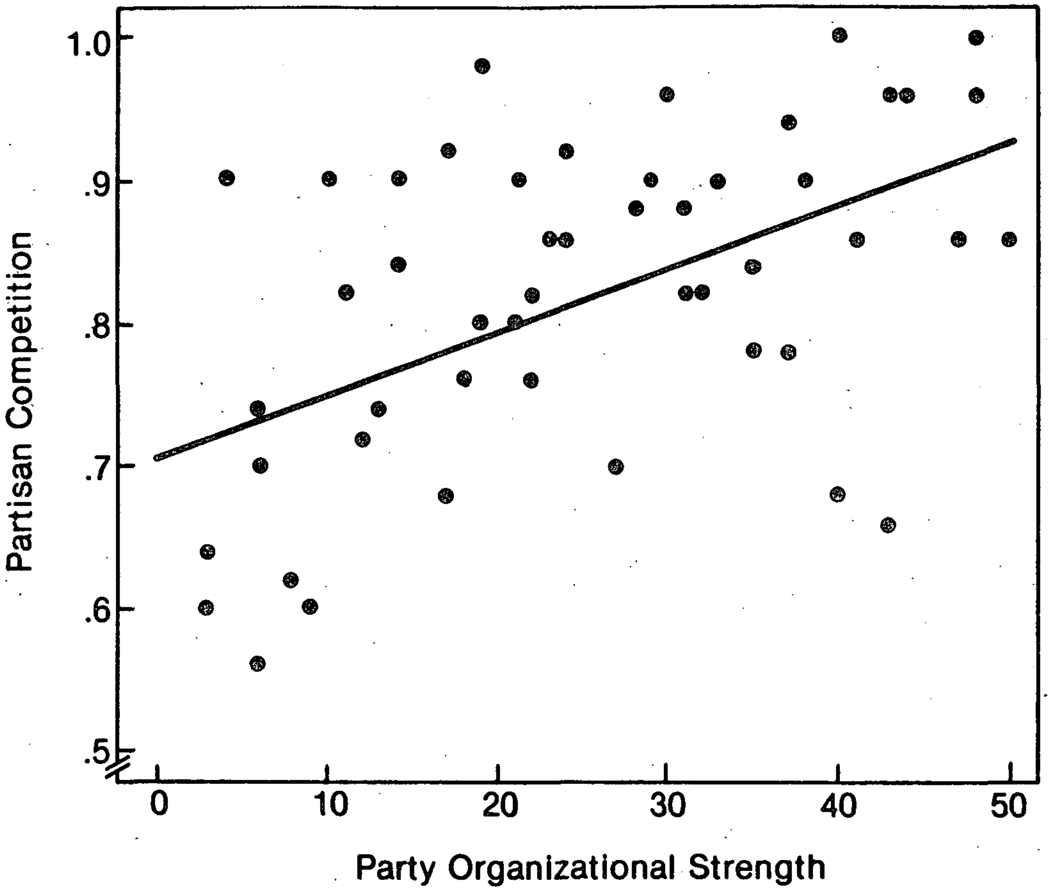
[5] To assess the status of party organizations at the local level, Gibson and his colleagues surveyed thousands of party leaders in American counties and received approximately 4,000 responses. The unit of analysis was the organization, not the individual leader. These party leaders, first of all, reported on the ability to fill key posts in the leadership, maintenance of organization during the electoral period and between elections, formalization of structure, professional staffing, and continuity of structure between elections, a constellation of attributes that Gibson and his associates label organizational structure. Second, on the dimension Gibson et al., call "programmatic activity," leaders testified about levels of contact and coordination with the organizations of candidates, campaigning, involvement in patronage, effects of precinct-level organization, pre-primary endorsements, and the recruitment of candidates. Statistical analyses indicate that these two dimensions, although conceptually quite distinct, are as an empirical matter representative of a larger, single concept, local party organization. In Table 8, Gibson et al. (1982) present an average score for each state's local Democratic and Republican party organizations, a figure derived from factor scores for individual counties. For a similar analysis of state party organizations, see Gibson et al. (1983).

[6] More specifically, we took the ranks of each state's Democratic and Republican parties on organizational structure and averaged them for scores that range from 1 through 50.

[7] It is also quite plausible that, faced with stiff partisan competition, leaders respond with greater organizational efforts. Or perhaps partisan competition and organizational strength both derive from the same set of influences. We have considered these two propositions long and hard, but we have rejected them on both theoretical and empirical grounds. First, with few exceptions, the literature we have unearthed speaks of organization as a determinant of competition—not the other way around. Second, as a precaution, we have performed two-stage least-squares analyses as a means of detecting simultaneity between organizational strength and partisan competition, and we found no evidence that threatened the validity of the results we report in this article.

Figure 4. Party Organizational Strength and Partisan Competition



tion of region and organizational strength—is captured in the following equation:

$$PC = b_0 + b_8 PS + b_5 R + e,$$

where *PS* represents organizational strength and the other notation remains the same. Naturally, we expect b_8 and $b_5 > 0$. Estimating these parameters produces the following result:

$$\underset{(23.08)}{PC = .652} + \underset{(2.23)}{.002PS} + \underset{(4.37)}{.143R} + e.$$

For both region and organizational strength, the coefficients are statistically significant, fortunately they run in the correct direction, and the $R^2 = .49$, nominally better than the explanatory potency of region alone.

The Full Model of Partisan Competition

None of the partial explanations for variations among the states in partisan competition particularly satisfies us, partly because individual variables in some cases do not carry much weight or because the fit of the partial model is not impressive, but mainly because we need to assess the influence of the major set of variables in one summary equation. Accordingly, we now focus on a model in which we combine the four avenues—socioeconomic, urbanization, party organizational, and regional—in a single analysis. Using the variables as previously described, we show the results of the full-blown multivariate assessment in Table 2.

In the full model shown in Table 2, we see that the more captivating specification provided in the multivariate context markedly strengthens the socioeconomic influences that had served as important predictors in the partial analysis presented earlier. Income and income inequality once again fall short of an independent relationship to partisan competition. Both income level and income inequality are individually correlated with partisan competition ($r = -.49$ and $r = -.34$, respectively), but neither is significantly nested in the multivariate model. It just turns out that differentials in income among states do not independently contribute elements of heterogeneity supportive of competitive political parties. Such differences are, in fact, taken up by other socioeconomic realities, mainly interstate variations in levels of educational attainment and differences in the extent of homeowner occupancy.

Dissimilarities among states in partisan competition depend very heavily upon differences in the states' educational levels. This dependency, not so sharply clarified in the earlier, partial analysis, emerges in bold relief in the full multivariate specification. If we control for the other relevant variables, including striking differences between northern and southern states in the extent of citizens' educational attainment, the relationship between education and partisan competition is very robust and imparts the largest of the standardized regression coefficients ($\beta_1 = -.53$). In short, then, differences among the states in the proportion of citizens who attain at least a high school education, or fail to do so, manifest very strongly the effects of socioeconomic diversity on partisan competition. Furthermore, variations in levels of homeowner occupancy, too, evidence the impact of socioeconomic diversity, although the influence of this variable on partisan competition is not so trenchant as for education (for owner-occupancy, $\beta_4 = .26$).

Urbanization has so far proved disappointing in helping to acount for divergences among the states in competition between parties. Little of the conventional wisdom seemed valid to us upon the occasion of our earliest analyses of the data on competitiveness; as we have observed, zero-order correlations between several measures of states' degrees of urbanization on one hand and partisan competition on the other were negligible. Some very attractive hypotheses about the effects of urbanization hardly got off the ground in our analysis. For instance, the appealing and trendy notion that population growth, epitomized by the booming explosion of migration to the Sunbelt, has fostered increases in partisan competition does not hold up on examination. Growth in population is not correlated with partisan competition in the 1970s ($r = -.08$), and it does not enter usefully into any of our multivariate models. Despite discouragement, we believed that some indication of urban heterogeneity, beyond the greater socioeconomic diversity characterizing urban states, was required for a fully pleasing theoretical account of partisan competition.

Thus, although the preliminary multivariate estimates of the influence of urbanization alone did not prove encouraging, we retained the proportion of the states' populations living in SMSAs and the size of populations in the all-embracing model. Table 2 indicates how propitious that retention was. Size of population comes to have an especially weighty role in monitoring the diversity of the states which, in turn, nourishes com-

Table 2. The Effects of Socioeconomic, Urbanization, and Political Variables on Partisan Competition

	Regression Coefficients			
Variables	Unstandardized	Standardized	*t*-ratio	Significance
Intercept	.559		.97	n.s.
Socioeconomic				
Education	−.819	−.53	−3.01	$p < .005$
Income	−.014	−.01	− .04	n.s.
Income inequality	.174	.02	.17	n.s.
Owner occupancy	.560	.26	2.10	$p < .025$
Urbanization				
Metropolitan population	−.108	−.22	−1.56	$p < .10$
Population size (in millions)	.007	.29	2.55	$p < .01$
Political				
Party organizational strength	.003	.37	2.86	$p < .005$
Control				
Region	.052	.19	1.34	$p < .10$

$N = 50$; $R^2 = .68$; $df = 41$.

petition between parties. Of the alternative influences, size of population ranks as third most important (β_7 = .29). It has been shrewdly observed that "it is difficult to sort out the unique effects of size (population) on diversity, dissent, and party competition, because more populous units tend to be not merely larger but more urbanized and 'modern' " (Dahl & Tufte, 1973, p. 95). Yet our indicator for the magnitude of the metropolitan populations of the states continues a moderately significant hold on variation in partisan competition, although its influence upon partisan competition is negative (β_6 = −.22).

Nested in the bosom of controls for region, socioeconomic status, and population size, metropolitanization induces uncompetitive politics. V. O. Key was almost surely right to have thought that states with a clear metropolitan-rural cleavage would be more likely to exhibit competitive partisanship than the more homogeneous states. Nowadays, most states have at least one metropolitan area, and in many states an overwhelming proportion of the people live within the boundaries of a metropolitan area. In 1980, in 34 states, more than half the population lived in a metropolitan area, and in 23 states more than two-thirds had a metropolitan residence. The political experience of large American cities does not teach us that metropolitan ecology conduces to competitive politics. On the contrary, the history of urban politics is shot through with evidence of noncompetitive politics—boss rule, the political machine, and one-party dominance over long periods of time. Politically safe public offices have become a growth industry in many of the great cities of the nation. Of course, becoming more urban may have helped rural states, at some point in their development, become more politically competitive. But, on the basis of these results, a state once metropolitan is, on average, likely to become less competitive, although the effect in its own right is not very great.

That well-organized political parties encourage and facilitate political competition constitutes an intuitively pleasing notion. Socioeconomic diversity and the multitude of variations beyond the merely socioeconomic, encouraged by size, deeply engender competitive party politics. But the mobilizing energies of organized political parties activate and maintain competitive politics. Parties, after all, involve themselves in recruiting and nominating candidates, raising money, conducting campaigns, organizing elections, and the rest. To the extent parties at the grass roots engage in organized politics—to the extent they *actively compete* one with another—competition will characterize politics.

This lesson we learn well from the results of our multivariate model. "Organizational strength" monitors the institutional clout and purposive political activity of county party organizations in the states. Even in the entourage of the full model, organizational strength has a powerful impact on partisan competition; among the variables in the model, it has the second largest beta weight (β_8 = .37). In retrospect, we see that to have attempted to understand variations among states in partisan competition without taking the active politics indicated by a measure of party organizational strength into account would, indeed, have been folly.

Finally, in spite of the excellent performance of the array of variables already included in our model, a regional effect persists. Although modest, the North-South dummy variable continues to produce a mildly significant relationship to competition (β_5 = .19). The results for the full model in Table 2 and for the earlier formulations, taken together, indicate that regionalism has an impact, in the main, because of the quite different demographic profiles of the South and North. Still, it is clear that politico-cultural phenomena separated by the great divide of the Mason-Dixon line continue to have some effect upon our political life, although this regional effect has doubtless diminished substantially since the 1950s.

Summary and Conclusion

We have taken at least a modest leap forward in the development and testing of an explanation for variations in partisan competition among the American states. In this attempt to account for differential competitiveness, we have called on and assessed many variables, but, in the glare and pressure of a multivariate statistical analysis, we have chosen few. Some—religious diversity, population density, ethnic diversity, population growth—are not correlated with partisan competition at the zero-order and could not establish themselves in a multivariate framework. Others, such as income or income inequality, lost explanatory usefulness in the multivariate model. We have demonstrated unmistakably the substantial effect of socioeconomic diversity, size of population, region, and party organizational strength on variations in partisan competition. These variables together provide a model that handsomely befits the variance in competition between political parties in the states (R^2 = .68).

No one doubts for a moment that partisan competition has a profound grounding in socioeconomic complexity, in the sheer size of political units such that the probability of opposing coalitions is enhanced, and in the politico-cultural differences between regions. What is equally important and often overlooked (partly because most of the time we have lacked measurements of it) is that active effort by organized political parties contributes extensively to the generation of competitive politics. This is no tautology; competitive politics has sometimes existed even when the contending political parties were weak, or even virtually nonexistent. Nevertheless, the organizational strength of political parties (how well they are organized and how active they are) is a factor of major importance in understanding partisan competition. Further, we can make our politics more competitive. The appropriate heterogeneity of the political community may unfold too slowly for our taste, and in some vital ways be beyond effective human manipulation. But organizing political parties at the grass roots and making them active in the conduct of nominations, politicking, and elections are within quite immediate human reach. The worst enemy of competitive democratic politics is communal lethargy.

Political scientists need to explore more thoroughly than heretofore the public benefits of competitive party politics. Partisan competition has a striking tendency to increase the turnout of voters at the polls. If we calculate the average turnout in each state for elections in the 1970s for president, U.S. representative and senator, and governor, our partisan competition scores are highly correlated with average turnout ($r = .64$). Quite apart from other relevant considerations, competition retains a significant influence upon turnout (see Caldeira & Patterson, 1982a; Patterson & Caldeira, 1983). Beyond enhancing turnout, partisan competition begets healthy political activity, fostering more vigorously fought campaigns, enhancing the vitality of primaries, encouraging responsive leadership (see Caldeira & Patterson, 1982b; Jewell & Olson, 1982, pp. 120-126, 205-209). Moreover, there is some evidence that competition between cohesive political parties conduces more responsible policymaking and perhaps particular kinds of policies (e.g., Morehouse, 1981, pp. 27-41). But in addition partisan competition surely must engender a higher incidence of split-ticket voting and divided partisan control of state governments (e.g., Jewell & Olson, 1982, pp. 232-238).

Partisan competition is no panacea. V. O. Key did not think that all of the problems of politics in the American South, or in any of the other states, would be taken care of if they could all become politically competitive (see Key, 1949, pp. 302-310). It is, of course, within the realm of possibility that men and women may make politics more competitive, but if so, we should remember that "political competition may be a condition precedent to government in the popular interest; it does not invariably produce that result" (Key, 1964, p. 299).

Appendix

This analysis focused on elections for governor and on the memberships of the houses of the state legislature in the American states during the 1970s. At least three gubernatorial elections occurred in every state during these years. In nine states (Delaware, Indiana, Missouri, Montana, North Carolina, North Dakota, Utah, Washington, and West Virginia), the elections for governor took place in the presidential election years—1972, 1976, and 1980. In 27 states, the gubernatorial elections were conducted in the congressional midterm years—1970, 1974, and 1978. In four states (Arkansas, New Hampshire, Rhode Island, and Vermont), governors were elected every two years during the 1970s, and these elections were held in the even-numbered years. In five states (Kentucky, Louisiana, Mississippi, New Jersey, and Virginia), the gubernatorial elections occurred in odd-numbered years. Finally, the periodicity of gubernatorial elections changed during the 1970s, in four states from every second year to every fourth year (Iowa, Kansas, South Dakota, and Texas), and in Illinois from the presidential to the midterm years. Altogether, our analyses encompassed 166 gubernatorial elections. Gubernatorial election results we drew from *America Votes 14* (Washington, D.C.: Congressional Quarterly Press, 1981).

The partisan distribution of seats in the houses of the state legislatures was calculated for the six even-numbered years between 1970 and 1980, except that 1970 had to be excluded for Minnesota because in that state partisan elections did not commence until 1972, and Nebraska had to be excluded altogether because of its nonpartisan legislature. The data on party membership in the legislatures were drawn from the *Statistical Abstract of the United States: 1975*, p. 448, and the *Statistical Abstract of the United States: 1981*, p. 494.

Our dependent variable, the partisan competition score, was derived by computing the mean of the average percentage of votes won by Democratic candidates for governor, the average proportion of Democratic seats in the state legislatures (calculating houses and senates separately), and the average percent of terms for governor and legislative houses in which the Democrats controlled. The partisan direction was removed from these scores by subtracting those above .5 from 1.0, and then adding .5 to all the scores. This "folded" scale ranged in magnitude from .5 to 1.0.

We have derived and measured the independent variables included in the regression analyses as follows:

Education (*E*): Percentage of the 1980 population who attended school for fewer than 12 years.

Income (*I*): Percentage of households with incomes below $10,000 in 1980.

Income inequality (*IE*): Gini index of income inequality for 1980 income distributions.

Owner occupancy (*O*): Percentage of occupied housing units which were owner occupied in 1980.

Metropolitan population (*M*): Percentage of the population residing in Standard Metropolitan Statistical Areas in 1980.

Population size (*P*): Total 1980 population in millions.

Party organizational strength (*PS*): Scores measuring the strength of local organizations for each state were ranked for each party, and average ranks were calculated for each state.

Region (*R*): Coded 0 for southern states, and 1 for nonsouthern states.

Data for home owner occupancy, population size, and income come from the *1980 Census of Population and Housing: Supplemental Report* (Tables H-1, P-1, and P-4). Data for education and the metropolitan population come from the *Statistical Abstract of the United States: 1981*, pp. 17, 144. Measures of the strength of Democratic and Republican local party organizations in the states are drawn from Gibson et al., 1982.

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A New Theory of Congressional Compromise

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A new model of congressional compromise is constructed in which leadership, faced with a bill whose prospects are uncertain, is forced to compromise. Leadership has policy preferences of its own and seeks a compromise that will minimize its expected loss.

A reasonable expression for the probability of each outcome is assumed, and the form of the optimal compromise is derived based on two simple parameters. The solutions are quite revealing and support the expectation of compromises close to a 50-50 split under a broad range of conditions.

Compromise is an essential ingredient in the congressional decision-making process. The need for multiple majorities to pass legislation usually requires that a bill be crafted so as to be acceptable to most affected groups. Textbooks on Congress refer repeatedly to this phenomenon. However, there is little systematic analysis of how the process of compromise works. Studies of House-Senate conference committees (e.g., Fenno, 1966; Ferejohn, 1975; and Strom & Rundquist, 1977) focus on the question of which side wins and not on the shape of the final compromise. Yet if we are to understand the process of compromise, we must to some degree be able to predict the split embodied in any given compromise.

This article offers a model that will allow us to make predictions about the form compromise will take. Specifically, compromise will be seen as occurring through the amendment process. Suppose a bill is supported by the majority or minority leadership in either house of Congress. Before the vote on final passage, leadership becomes convinced that the bill's chances of success are dim. What will the leadership do? They may wish to withdraw the bill and perhaps reschedule it for floor consideration at some later date. Or, they may prefer to offer a compromise amendment in an effort to improve the bill's chances of passage. The leadership may not wish to compromise the bill, but they may feel that half a loaf is better than none.

Can we say anything about how much the leadership will sacrifice? The model to be introduced in the next section will answer this question. We will assume that the alternative forms of the bill can be represented by a one-dimensional continuum with the unamended bill and the status quo as two end points. Leadership will have its own policy preferences, represented by a loss function defined over this interval. For specificity we will assume quadratic loss, but similar results can be obtained when the loss function has a different shape. Given the leadership's estimate of the likelihood that the unamended bill will win on final passage, it is then possible to say precisely how much the leadership will give up through the amendment process if leadership's objective is to minimize its expected loss with respect to the set of final outcomes.

The model derived in the next section does not yield a closed-form solution to the compromise problem. Accordingly, the following section contains computer-generated results for an interesting set of cases. The solutions are easily interpretable and demonstrate the utility of the model. Most of the results cluster around a 50-50 compromise.

The Model

I will keep the model as simple as possible in an effort to reflect congressional realities. It serves little purpose to build a model so complicated as to require an inordinate amount of information on leadership's part. The model to be presented is based on a minimal set of informational requirements.

To begin with, we assume that alternative forms of the bill in question can be represented as points on a one-dimensional continuum. This assumption is now commonplace in the congressional literature and is strongly supported by empirical evidence (e.g., Clausen, 1973; Jackson, 1974; MacRae, 1958; Poole, 1981; Schneider, 1979). Appropriations bills clearly meet this criterion, but the assumption of unidimensionality is much more general. No matter how complex a bill, it is usually possible to represent it somewhere on a

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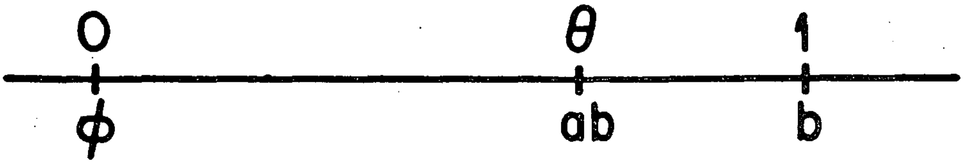
Figure 1. A One Dimensional Policy Scale



left-right scale. This scale may be a general ideological scale (e.g., Poole, 1981; Schneider, 1979) or it may represent a policy domain (e.g., Clausen, 1973; MacRae, 1958). The net result is the same. Modifications of any single bill consist of moving the bill on an interval scale.

Such a scale is portrayed in Figure 1. The bill in question is denoted by $b = 1$, which is the form of the bill before leadership decides to offer a compromise amendment. The form of the compromise is denoted by $ab = \theta$, which is the bill b as altered by the amendment a. In other words, the amendment a moves the bill from $b = 1$ to $ab = \theta$. Finally, the status quo, or no bill, is denoted by $\phi = 0$. The choice of 0 and 1 as locations for ϕ and b is strictly for computational convenience. Thus, negative values on the policy scale are of no particular significance (i.e., they do not signify "negative" bills).

Figure 2 represents the decision tree that char-

Figure 2. The Structure of the Voting Process

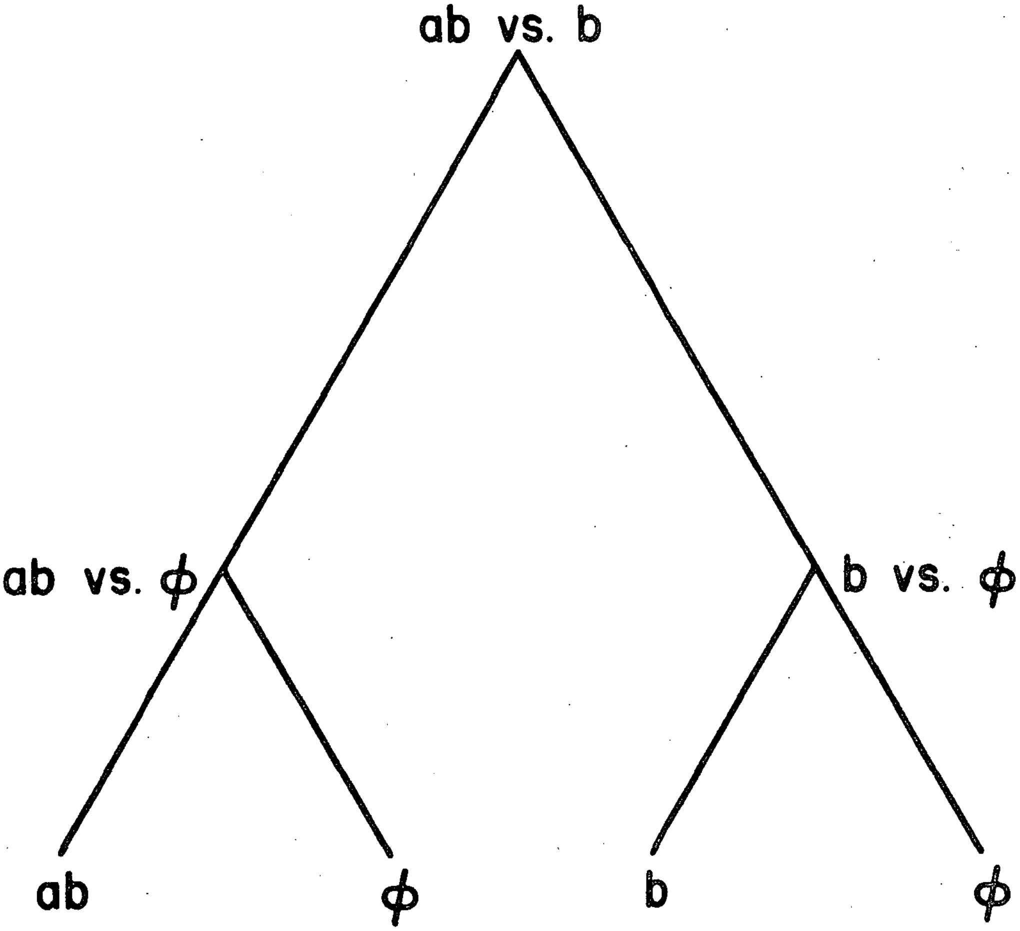
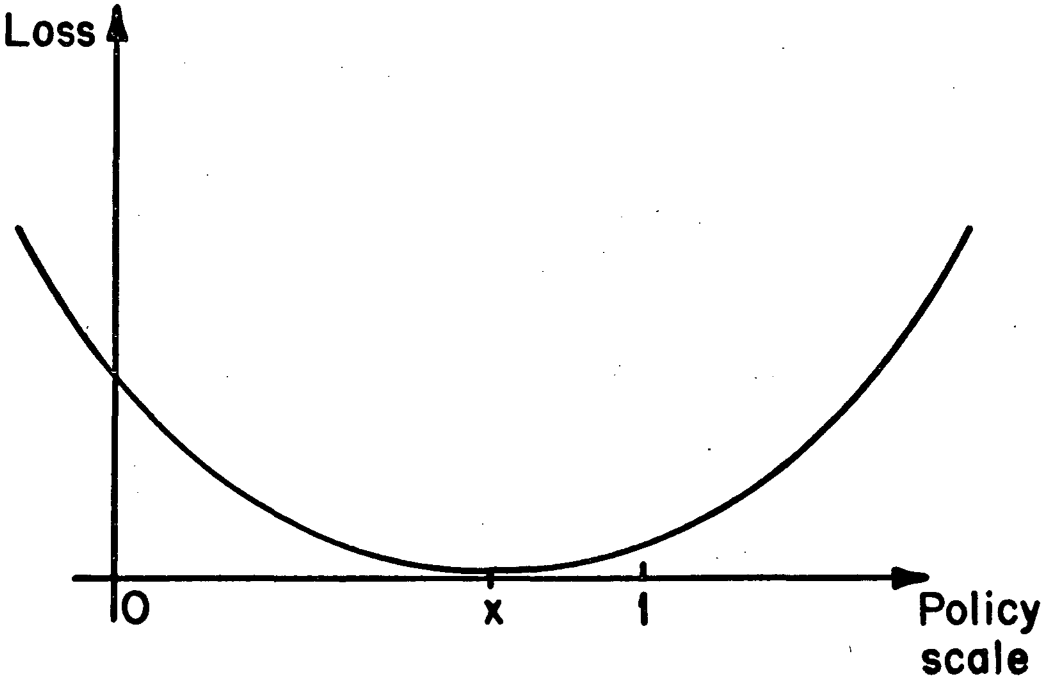


Figure 3. Leadership's Preferences over Alternative Policy Outcomes



acterizes the voting process. If no amendment were offered, a simple vote between *b* and ϕ would occur. It is leadership's uncertainty about the outcome of such a vote that leads them to offer the amendment *a*. Once offered, the amendment is voted on before the vote on final passage of the bill. Thus, the first vote is on whether or not to adopt *a*. If the amendment is adopted, the amended bill *ab* is voted on against ϕ in the final vote. The first contest, then, is between the bill with the amendment (*ab*) and the bill without the amendment (*b*). If $\theta \neq 1$ or 0, the three possible final outcomes are $\phi = 0$, $ab = \theta$, and $b = 1$.

Although *b* may have been previously amended, we need not assume that *a* is the only additional amendment that will be offered. Instead, all that is necessary to assume is that leadership is concerned solely with the effect of *a* on *b* at the time the amendment is offered. If other amendments are offered at some later point, leadership either does not anticipate that this will happen or is unaware of the form that these amendments will take. Alternatively, such amendments may be trivial as far as the bill's substance is concerned or may not affect the bill's chances of passage. In short, there are a number of behavioral justifications for limiting the decision problem to ϕ, *ab*, and *b*. However, my approach can be generalized to allow for additional amendments whose form is known to the leadership in advance of their decision to offer *a*.

Assume that leadership has policy preferences over the policy scale depicted in Figure 1. Specifically, assume a quadratic loss function, such as that portrayed in Figure 3. Thus, a point such as $x \epsilon R$ on the policy scale represents the most preferred form that the bill can take from leadership's standpoint, with alternative forms being less preferred at an increasing rate the more they differ from *x*. As can be seen in Figure 3, leadership may not view *b* as its ideal bill, but still finds it preferable to ϕ, the status quo. There is no need to assume that $x \epsilon [0,1]$, but for the examples I present in the following section I will set $x = 1, .75$, and .5. These values will allow us to examine a set of cases for which I would expect leadership to vary in its willingness to compromise.

The justification for assuming a single preference function for leadership rests on congressional evidence. If party leadership is divided on the desirability of various forms of a given bill, the chances decrease that leadership will play an active role in attempting to shape the bill. As Dodd and Sullivan (1979, p. 132) point out in their study of partisan vote gathering in the 93rd Congress, "Party leaders and members of the whip system believe that they cannot be involved in the indiscriminant alteration of committee legislation." As a result, party leaders feel it is necessary to secure the cooperation of the committee chair and key committee members before proceeding. In short, a united leadership (including committee leaders) is usually a necessary prerequisite for leadership intervention in the process of policy formation.

This statement does not deny the existence of extensive negotiation and bargaining among party and committee leaders before a decision is made to offer a compromise amendment. The Steering and Policy Committee decides whether the Democratic party will take a position on pending House legislation. The Republican Policy Committee performs the same function for House Republicans. Spirited debate among members of either of these committees (both of which include all of the top party leaders) does not preclude the possibility that the leadership will unite behind a strategy to work for a specific legislative goal.

The important point to keep in mind is that the decision by leadership to intervene in the policy-making process is not made lightly. Dodd and Sullivan (1979, p. 132) list three factors that are necessary for leadership to intervene: a high degree of certainty that the unamended bill is in trouble, a strong belief that amending the bill will significantly help its chances of passage, and cooperation on the part of the committee chair and "relevant" committee members.

Let $\tilde{\theta}$ denote the random variable, which takes as its values 0, θ, and 1. We will use the index $s = 1, 2, 3$ to denote these three values so that $\tilde{\theta}_1 = 0$, $\tilde{\theta}_2 = \theta$, and $\tilde{\theta}_3 = 1$ is an alternative representation of $\tilde{\theta}$. Finally, let π_s represent the subjective probability as estimated by the leadership that $\tilde{\theta}_s$ will be the ultimate outcome of the voting process. Assuming quadratic loss, I posit the objective of leadership to be the selection of θ that will minimize expected loss. Or, formally

$$\min_{\theta} E(\tilde{\theta} - x)^2, \tag{1}$$

where

$$E(\tilde{\theta} - x)^2 = \sum_{s=1}^{3} \pi_s (\tilde{\theta}_s - x)^2 = \pi_1(\tilde{\theta}_1 - x)^2 + \pi_2 (\tilde{\theta}_2 - x)^2 + \pi_3 (\tilde{\theta}_3 - x)^2 = \pi_1 x^2 + \pi_2 (\theta - x)^2 + \pi_3 (1 - x)^2.$$

In words, I assume that leadership selects the amendment that will, in its opinion, produce the smallest average loss in terms of the final policy result.

To reach a solution to this problem I must specify the functional form of π_1, π_2, and π_3. Clearly this is not a trivial undertaking. For example, π_1 is leadership's estimate of the likelihood that the status quo will be the final outcome. This parameter would logically appear to depend on θ (the form which the compromise takes) and the likelihood that the status quo can beat the unamended bill b. Similar considerations apply to the problem of estimating π_2 and π_3.

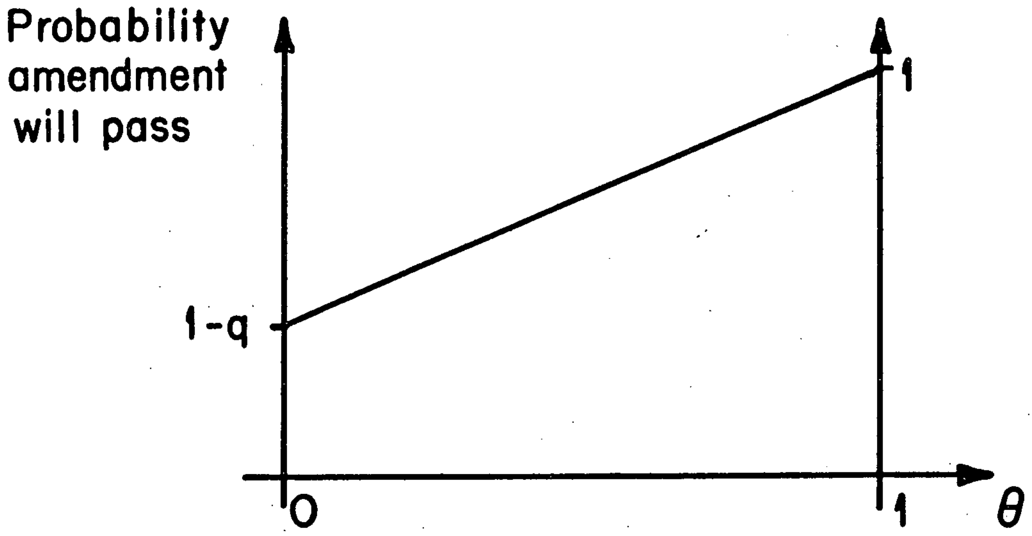
It might appear that I am creating a problem of infinite regress. However, consider the following solution. Let q denote the probability as estimated by leadership that $b = 1$ will beat $\phi = 0$ on final passage (i.e., the probability that the bill will pass if it is *not* amended). Since the contest between b and ϕ does not involve θ, there is no reason to make q a function of θ. The probability that $\phi = 0$ will beat $b = 1$ on final passage is, then, $1 - q$. In other words, if leadership does nothing to improve the bill's chances of passage, they estimate that the bill will pass with probability q and fail with probability $1 - q$.

If leadership offers a compromise amendment, the first vote contest is between $ab = \theta$ and $b = 1$. What is a reasonable representation of the likelihood that θ will prevail in this first contest (i.e., that the amendment will pass)? Figure 4 provides an answer. If $\theta = 0$, the amended bill is identical to the status quo and so should have the same chances of winning on the first contest as the status quo has of beating the unamended bill on final passage (i.e., $1 - q$).

Assume that the most preferred policy of a majority of the voters lies between 0 and 1. If all voters have single-peaked preferences and vote sincerely, then the closer θ is to 1, the better the chances that θ will beat 1. However, the assumption of sincere voting need not be made. Figure 4 concerns the likelihood that more votes will be cast for the amendment than against it and consequently is not tied to any single assumption about individual voting behavior. Given the difficulties in assessing individual vote intentions, it is reasonable for leadership to rely in part on past experience in determining the likelihood that alternative forms of the amendment will be adopted. Past experience is likely to show that the less an amendment seeks to change a bill, the more likely it is to pass. Thus, Figure 4 can be viewed as a reasonable summation of past and present information possessed by the leadership.

In recent years, floor voting in Congress has increasingly been fraught with uncertainties. As Sinclair (1981) points out in her study of the Speaker's task force, changes in the House during the 1970s have caused floor voting to become increasingly hard to predict. In fact, the use of ad hoc groups by the Speaker to work for passage of a specific bill or amendment is a result of this increased uncertainty about the outcome of floor votes. Thus, it makes sense for leadership to

Figure 4. The Amendment Vote (i.e., θ vs. 1)



adopt a simple rule like that of Figure 4, relating the likelihood that the amendment will pass to alternative forms that the amendment can take. Otherwise, leadership will be lost in the myriad complexities that can affect individual votes.

A similar consideration lies behind the linear relationship posited in Figure 4. If party leadership knew how everyone were going to vote, Figure 4 would actually be a step function, taking on only the values 0 and 1. Leadership uncertainty about vote outcomes argues against this assumption. Since the nature of leadership's uncertainty is difficult to define, a linear relationship between the form of the amendment and the likelihood that it will pass is a reasonable assumption. Such a relationship is certainly more credible than a power function, which would be more reflective of a modeler's imagination than anything likely to be used by a real-world decisionmaker. Although Figures 4 and 5 do not derive from a model of the median voter's most preferred policy, these figures are based on the idea that leadership is uncertain about the location of the median voter's most preferred policy.

This last point is important. If leadership knew the location of the median voter, all probabilities in the model would be 0 or 1. Thus, our analysis depends on leadership having, at most, probabilistic evidence concerning the location of the median voter.

The use of whip counts alleviates but does not eliminate leadership's uncertainty. Several points can be made about the information provided to the leadership by whip counts. First, as Ripley's (1964) study of party whip organizations points out, whip counts are partisan affairs. Information about the voting intentions of members of the other party must be obtained indirectly through lobbyists, executive branch officials, and other sources. For this reason, Ripley concludes that "information on Republican voting tends to be quite unreliable when it reaches the Democratic whip's office" (p. 247). Second, as Dodd and Sullivan (1979, p. 53) argue, whip counts are employed by the leadership as a "tool" and "not . . . as a prediction of the members' positions across all possible votes on the bill." In other words, whip counts provide partial information even about members of the leadership's own party. These counts typically reveal wavering or undecided members, and of course the results of these polls are used by the leadership to attempt to convert those opposed to its position. For all of these reasons, it is safe to conclude that whip counts do not eliminate leadership's doubts about future vote outcomes. These counts appear most useful in helping leadership estimate *q*, rather than providing leadership with a detailed road map of the relationship between the form of the amendment and its chances of passage.

The following argument supports setting the probability that the amendment will pass equal to 1 when $\theta = 1$. First, we assume that 1 is not the most preferred policy in the [0,1] interval of a majority of the voters (at least as perceived by leadership). Otherwise, compromise is unnecessary, and there is no problem to be analyzed. Thus, if θ is set just to the left of 1, we are assuming that θ will win with probability just less than 1. Given our assumption that passing the amendment becomes easier the less the amendment seeks to change the original bill, this conclusion makes sense. Congressional rules against dilatory motions require that θ be noticeably different from 1.

We thus arrive at the conclusion that $1-q(1-\theta)$ is leadership's estimate of the likelihood that the amendment will pass (i.e., that θ will beat 1). We now must specify the function that determines the likelihood that $ab = \theta$ will beat $\phi = 0$ on final passage. Figure 5 provides an answer. Many of the same arguments used to support Figure 4 are again applicable. If $\theta = 1$, then $\theta = b$ so that θ will have the same chance of beating ϕ as b has (i.e., q). With the assumption of single-peaked preferences and a majority of ideal points in the [0,1] interval, sincere voting dictates that as θ moves closer to 0, θ stands a better chance of beating 0. Since we are now analyzing a vote on final passage, sincere voting is the only reasonable assumption to make (assuming that voters are not thinking ahead about future bills). However, it is again important to note that Figure 5, like Figure 4, is an assumption about vote outcomes rather than individual voting. Thus, what is actually being assumed is that the greater the difference between the bill and the status quo, the harder it is to pass the bill. Like our assumption about amendment votes, past experience is likely to support this conclusion.

Finally, we assume that 0 is not the most preferred policy of a majority of the voters, since otherwise compromise is futile. Thus, if θ is set just to the right of 0, θ will beat 0 on final passage with probability approaching 1. The linearity assumption rests on the same justification used earlier. Leadership is uncertain about where voter ideal points are located and so has only a probabilistic estimate of the location of the median voter.

We have already set up a tension in the model. If θ is set close to 1, the amendment is almost sure to pass; but for θ to win on final passage, it should be set close to 0. As will be shown, this tension in conjunction with leadership's own policy desires leads to interesting results about the form of the optimal compromise.

We now have that θ will beat 0 on final passage (i.e., that the amended bill will win on final passage) with probability $1 - \theta(1-q)$. Given leadership's estimate q that b will beat ϕ on final passage, we can now derive π_1, π_2, and π_3. This derivation is most easily followed by referring to Figure 6.

The probability π_1 describes the likelihood that $\phi = 0$ will be the ultimate outcome of the voting process. As Figure 2 shows, there are two ways this can happen. The amended bill $ab = \theta$ can beat $b = 1$ and then lose to $\phi = 0$, or ab can lose to b, which in turn loses to ϕ. The probability of

Figure 5. The Final Vote if the Amendment Passes (i.e., θ vs. 0)

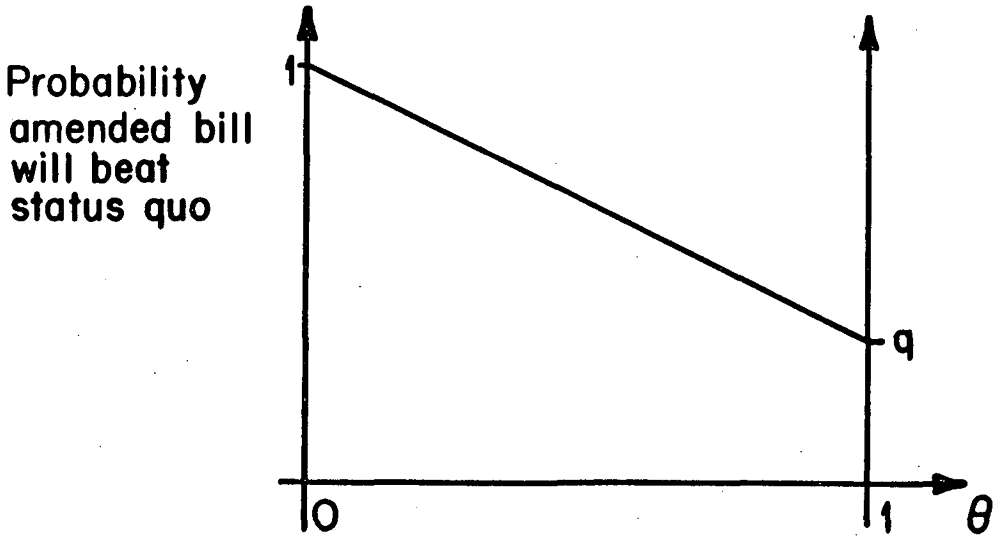
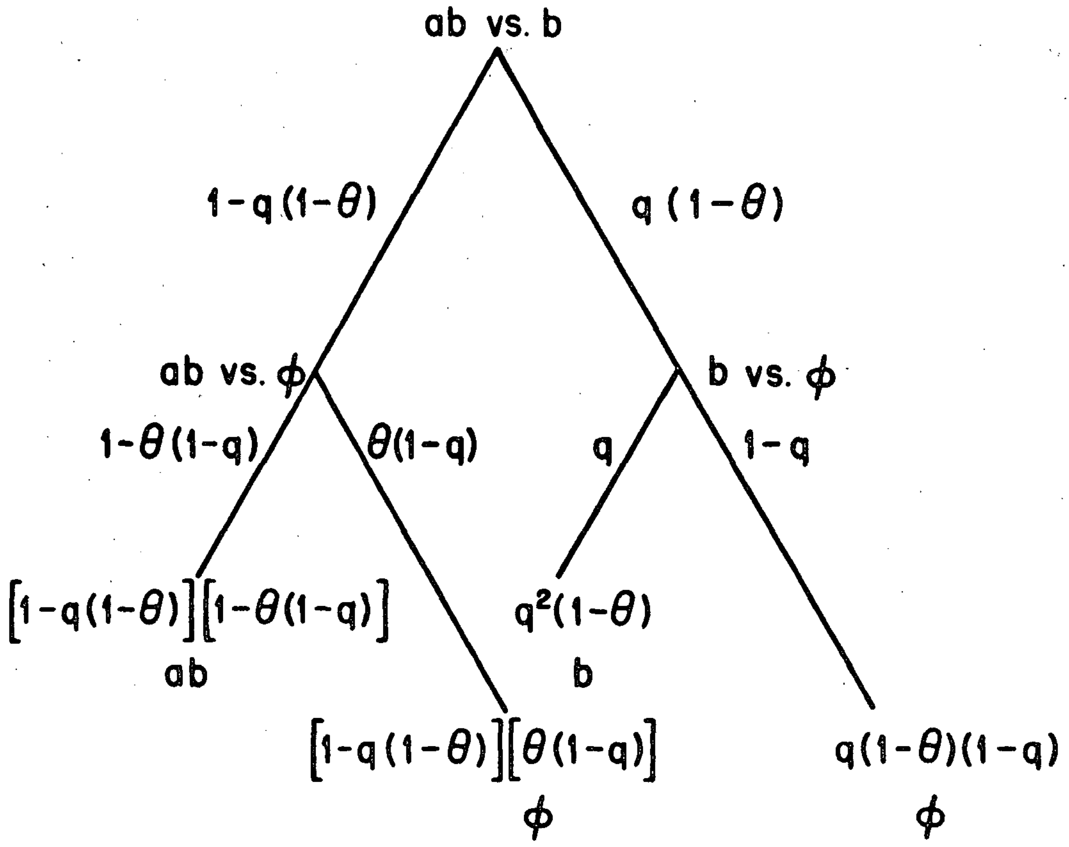


Figure 6. The Probabilities of Alternative Vote Outcomes



the first event is $[1-q(1-\theta)][\theta(1-q)]$, and the probability of the second is $[q(1-\theta)][1-q]$. Thus, $\pi_1 = [1-q(1-\theta)][\theta(1-q)] + [q(1-\theta)][1-q] = (1-q)(\theta+q(1-\theta)^2)$ is the probability that the status quo ϕ will be the final outcome of the voting process.

The probability that θ will be the final outcome, π_2, is easily seen from Figure 6 to be $[1-q(1-\theta)] \times (1-\theta(1-q)]$. This expression represents the probability that *ab* will beat both *b* and ϕ. It is interesting to note that the amended bill stands at least as good a chance of winning on final passage as the unamended bill. This follows from noting (see Figure 6) that $1-\theta(1-q) \geqslant q$, since $1-\theta \geqslant q(1-\theta)$.

Finally, π_3, which is the probability that the unamended bill will be the final outcome, is $q^2(1-\theta)$. This expression represents the probability that the amendment fails and that the unamended bill wins on final passage.

We can now evaluate expression (1) in order to derive the θ (i.e., the compromise amendment) that minimizes leadership's expected loss. Substituting for π_1, π_2, and π_3 we have that

$$\begin{aligned} E(\tilde{\theta}-x)^2 &= x^2[(1-q)(\theta+q(1-\theta)^2)] \\ &+ (\theta-x)^2\,([1-q(1-\theta)][1-\theta(1-q)] \\ &+ (1-x)^2\,[q^2(1-\theta)]. \end{aligned} \tag{2}$$

If we differentiate expression (2) with respect to θ and set the result equal to 0, we have that

$$\begin{aligned} \frac{\delta E(\tilde{\theta}-x)^2}{\partial\theta} &= \theta^3(4q^2 - 4q) \\ &+ \theta^2(-6q^2x + 6qx - 3q^2 + 9q - 3) \\ &+ \theta(4q^2x - 12qx + 4x - 2q + 2) \\ &+ 2q^2x - q^2 + 2qx - 2x = 0. \end{aligned} \tag{3}$$

Since expression (3) is a cubic function, it does not have a closed-form solution. If a linear loss function were posited, the first derivative would be a quadratic function, and so the first order condition could be derived by use of the quadratic formula. However, the resulting solution is no more intuitively interpretable than the solution to expression (3). As a result, we will pick some intuitively reasonable values for q and x and minimize expression (2) by evaluating it for all $\theta = .01, .02, \ldots, .99$. We can then look for patterns among the solutions to see how the optimal compromise behaves as a function of q and x.

Results

The values we choose for q are 0, .25, and .5. There seems little reason to analyze the problem for $q > .5$, since in this case the unamended bill is seen as more likely to win than lose, a circumstance hardly conducive to compromise. Recall Dodd's and Sullivan's necessary factor for leadership intervention that the unamended bill must clearly be "in trouble." Thus I choose to limit my analysis to cases where $q \leqslant .5$ on substantive grounds. It is not worth the expenditure of resources for leadership to intervene in the policy process if the unamended bill stands a better than 50-50 chance of passage without leadership's help. When $q = 0$, the unamended bill's chances are perceived as hopeless, whereas if $q = .5$, the unamended bill's chances are seen as a toss-up.

The values we choose for x are 1, .75, and .5. In the first case, leadership most prefers the unamended bill as is. This appears to be the most interesting case, suggesting a story that b is the leadership's own bill but is perceived as too liberal (or conservative) to pass without modification. We would expect that the smaller q is, the more leadership will have to give up. Conversely, for fixed q, the smaller x is, the less leadership should have to give up relative to x.

Table 1 presents our results. Let $\theta^*(q,x)$ denote the optimal θ given probability estimate q and most preferred policy x. Our theoretical expectations are borne out. For fixed x, $\theta^*(q,x)$ increases as q increases. The more likely the unamended bill is seen as being able to beat the status quo, the less leadership has to compromise to minimize its expected loss.

In addition, for a given q, the closer x is to 1, the more leadership will have to give up relative to x. In other words, the more attractive the unamended bill is to the leadership, the more leadership will have to sacrifice relative to the bill it most prefers. Interestingly, $\theta^*(0,1) = .42$, which is to say that leadership should offer an almost even split, even though the unamended bill (which leadership most prefers) is seen as doomed. This result flies in the face of conventional wisdom, which dictates that the much stronger side in a two-way contest should reap most of the benefits. All the solutions in Table 1 lie between .33 and .69, with the majority of solutions between .41 and .55. This would seem to suggest that 50-50 splits are attractive under a variety of conditions.

It may appear that the optimal θ is symmetric, i.e., that $\theta^*(q,x) = 1-\theta^*(1-q, 1-x)$. For $q = 0,1$ this is true. For example: $\theta^*(1,0) = .58$, whereas $\theta^*(0,1) = .42$; $\theta^*(1,.25) = .61$, whereas $\theta^*(0,.75) = .39$; and $\theta^*(1,.5) = .67$, whereas $\theta^*(0,.5) = .33$. However, for $q \epsilon (0,1)$, this relationship may not hold. Thus, $\theta^*(.5,1) = .69$, whereas $\theta^*(.5,0) = .22$.

Figure 7 is a graph of the expected loss function for $q = .25$, $x = 1$. Since it is a weighted sum of convex functions, $E(\theta - x)^2$ is convex (downward). The graphs for the other cases are similar.

Table 2 lists π_1, π_2, and π_3 for various $\theta^*(q,x)$. The results are quite revealing. In each case, the probability π_2 that the amended bill θ will be the final outcome exceeds .5. The unamended bill, on the other hand, has a uniformly low probability π_3 of being the final outcome. The probability that the amendment will pass $(\pi_1^a + \pi_2)$ is extremely high, ranging from .75 to 1. In most cases this probability is .85 or greater.

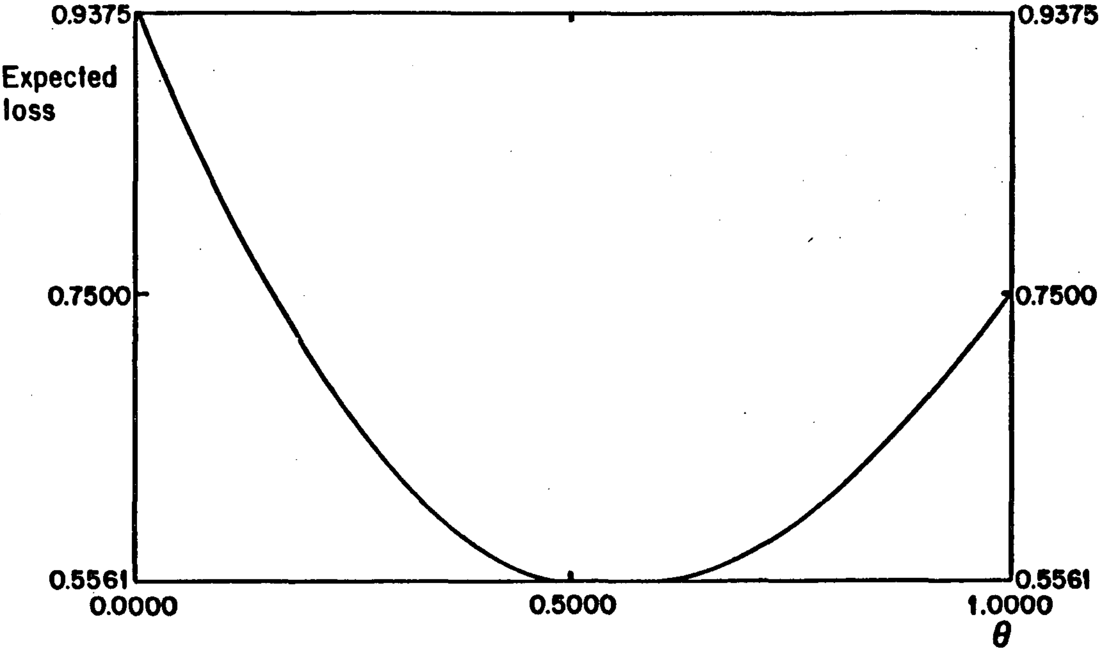
The final column of Table 2 $[1-\theta(1-q)]$ is the probability that the amended bill will beat the status quo on final passage. As can be seen, the amended bill always stands a better than 50-50 chance of winning on the last vote. Recall Dodd's and Sullivan's necessary factor for leadership intervention that amending the bill will significantly help the bill's chances of passage. As a

Table 1. Optimal θ for $x = 1, .75, .5; q = 0, .25, .5$

	x		
q	1	.75	.5
0	.42[a]	.39	.33
.25	.55	.50	.41
.5	.69	.61	.50

[a]Entry is optimal θ.

Figure 7. Leadership's Expected Loss for $q = .25$, $x = 1$, and $0 < \theta < 1$



comparison between the final column of Table 2 and the associated values of q indicates, the improvement in the bill's chances on final passage ranges anywhere from .16 to .67, depending on q and x. Other things being equal, the smaller q is, the greater the improvement. If the comparison is made between q and the chances that the amended bill will be the final outcome (i.e., π_2), the improvement is not as great but the same relationship holds. The smaller q is, the greater the improvement in the chances that a modified form of the bill will be the final outcome. The probability that *some* bill will be the final outcome is $\pi_2 + \pi_3$. If we view the improvement in the bill's chances from this perspective, the result still holds. The smaller q is, the greater the improvement in the chances that some bill (either *ab* or *b*) will be the final outcome.

What these results suggest is that the smaller q is, the more likely the leadership is to offer an

Table 2. Probability that Status Quo (0), Amended Bill (θ), and Unamended Bill (1) Will Be Final Outcome, i.e., (π_1, π_2, π_3), for Alternative $\theta^*(q,x)$

	$\pi_1 = \pi_1^a + \pi_1^{\bar{a}}$				
	π_1^a	$\pi_1^{\bar{a}}$	π_2	π_3	$1-\theta(1-q)$
$\theta^*(0,1) = .42$	.42	0	.58	0	.58
$\theta^*(.25,1) = .55$	.37	.08	.52	.03	.59
$\theta^*(.5,1) = .69$	.29	.08	.55	.08	.66
$\theta^*(0,.75) = .39$	.39	0	.61	0	.61
$\theta^*(.25,.75) = .50$	.33	.09	.55	.03	.63
$\theta^*(.5,.75) = .61$	.25	.10	.56	.10	.70
$\theta^*(0,.5) = .33$	.33	0	.67	0	.67
$\theta^*(.25,.5) = .41$	.26	.11	.59	.04	.69
$\theta^*(.5,.5) = .50$	.19	.13	.56	.13	.75

Note: π_1^a is the probability that the amendment will pass and the amended bill fail.

$\pi_1^{\bar{a}}$ is the probability that the amendment will fail and the unamended bill fail.

amendment to save the bill. No matter how we define improvement, the less the unamended bill's chances of passage, the greater the improvement in the bill's chances will be if a compromise amendment is offered.

Another conclusion is perhaps less intuitive. The smaller x is, the more likely the leadership is to compromise further. For fixed q, the degree of improvement in the bill's chances of passage increases as x decreases. In other words, the closer leadership's most preferred policy is to a 50-50 compromise, the more likely leadership is to give up more in an effort to gain passage of the bill (the one exception being for $\theta^*(.5,.5)$).

In sum, the results generated by our model of congressional compromise provide interesting insights into the question of how leadership makes trade-offs between perceived reality and its own policy desires. In some cases our results confirm common sense, but in other cases our results are counterintuitive. In either event, our results hardly could have been guessed in advance. The exact nature of the optimal trade-offs—how much leadership should give up based on the degree to which compromise is seen as necessary for the bill to pass—is hardly obvious. In the next section of the article, I will offer some additional comments on the empirical value of these results.

Discussion

What testable propositions can I derive from the theoretical results? The model I have constructed is actually quite simple; according to it, the compromise amendment that leadership will offer—to replace b with θ—is a function of its most preferred policy x and its perception of the probability q that the bill in its current form b will win on final passage. The best chance of gathering real world data on these three variables (θ, x, and q) would appear to be through a case study analysis of individual bills. Appropriations bills and budget resolutions come immediately to mind as the type of bill for which reliable estimates of θ and x are most likely to be available. Estimating q is more difficult, but our results are interpretable in terms of relative values of q. Thus, our finding that the smaller q is, the more likely the leadership is to amend the bill and the more it will sacrifice, are propositions that can be tested across a series of bills for which x is nearly the same. (Keep in mind that x is defined relative to b and ϕ and is not an absolute figure.) It should also be possible to test the proposition that leadership's willingness to compromise with respect to x is related to the degree to which x represents a compromise between b and ϕ.

Dodd and Sullivan (1979) argue that legislation benefitting one or two specific groups in a party's constituency is particularly prone to the need for compromise amendments in order to win on final passage. Farm legislation, consumer legislation, and labor legislation are examples cited among Democratic programs. A series of agricultural price support bills might then offer a fruitful data base for analyzing the form that compromise takes in different settings. Labor-supported trade bills designed to restrict foreign competition is another set of bills worth examining.

There is a different way of viewing the results reported in Table 1. Suppose leadership itself has difficulties in estimating q. For argument's sake, suppose that q is perceived as equally likely to be 0, .25, or .5. For a given x, the optimal θ is then $[\theta^*(0,x) + \theta^*(.25,x) + \theta^*(.5,x)]/3$. For $x = 1$, this expression is .55; for $x = .75$, it is .5; and for $x = .5$, it is .41. Thus, as uncertainty about q increases, the likelihood goes up that θ will be near .5. This is another result that is susceptible to verification. Since $\theta^*(q,x)$ increases as q increases, it is clear that this result would obtain for a fairly broad range of probability densities of q over $[0,.5]$. Following the histories of similar bills, we should find that as leadership's uncertainty about the bill's chances of passage increases, the more likely leadership will be to offer a 50-50 compromise. Degree of uncertainty cannot be measured formally, but it is sometimes possible to infer high or low degrees of uncertainty from statements made by members of the leadership or key committee members that are recorded either in the *Congressional Record* or by the press. (This approach is used in Enelow and Koehler (1980) and Enelow (1981).) Thus, the relationship between degree of leadership uncertainty and 50-50 splits is another testable result that is derived from our model.

It may also be the case that various factions in the leadership, although united behind $\theta^*(q,x)$, are in disagreement over x. In other words, all factions are in agreement that a single compromise amendment should be offered, but each faction prefers that the amendment be based on a different x. As one example, suppose that $x = 1, .75$, and .5 are the most preferred policies of three equally influential factions in the leadership. Then, for a given q, $[\theta^*(q,1) + \theta^*(q,.75) + \theta^*(q,.5)]/3$ is the optimal θ. For $q = 0, .25$, and .5, the optimal $\theta = .38, .49$, and .60, respectively. The average of these solutions is .49, which is the optimal θ when the three leadership factions perceive $q = 0, .25$, and .5 as equiprobable. Again, it is clear that an even split is nearly optimal for a broad range of cases.

Thus we conclude that as uncertainty over q increases, divisions within the leadership increase, or both, the likelihood increases that leadership will offer a 50-50 compromise.

Experimental methods offer another way of testing our model of legislative compromise. Since the optimal θ is a function of two simple parameters (q and x), it is easy to set up an experiment designed to see whether "leadership" selects the theoretically predicted θ. It is also possible to vary the shape of leadership's loss function to measure its effect on the compromise amendment selected. Since quadratic loss means that compromise becomes more costly the further it is from x, we should expect to find linear and concave (downward) loss functions associated with fewer 50-50 splits.

Conclusion

Compromise is a general phenomenon and not just something that occurs in legislatures. The model offered in this article is a fully general one that can be applied to a broad variety of cases of group decisionmaking. A motion is under consideration. Its sponsors become convinced that there is a significant chance the motion will fail. They then consider amending the motion to increase the chances of getting it passed. How should the motion be amended so that the motion's sponsors give up no more than they need to in exchange for significantly improving the chances of getting the motion passed?

This type of problem can arise in any organization or assembly operating under parliamentary law (e.g., *Robert's Rules of Order*). The sponsors of the original motion can be any subset of the deliberative body. I focus on party leadership because it is this group (along with committee leaders) that is most prominent in the process of congressional compromise. However, there are many instances in which an individual congressman plays the role we have given to party leadership. In such cases, the absence of leadership involvement makes the task of sounding out support and gathering votes a lot more difficult. On bills important to the leadership, a system exists, whether it be a Speaker's task force or the whip organization, for polling members and building a majority coalition. Thus we would expect compromise to be approached more systematically when leadership is involved.

In closing, I believe that the model of congressional compromise offered here is plausible and generates interesting results. As is true for election campaigns, those who watch Congress are usually interested in identifying winners and losers. Such a perspective masks the reality that, in most cases, the major actors win to some degree *and* lose to some degree. Congressional outcomes are rarely of the all-or-nothing variety. It is therefore appropriate to build models that reflect this reality and are also capable of generating testable predictions.

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Bureaucratic Agenda Control: Imposition or Bargaining?

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Niskanen's economic model of bureaucratic behavior assumes that bureaucratic agenda control allows the bureau to impose upon a passive legislature its most preferred alternative from among the set of alternatives that dominate the status quo. But does a monopoly on the agenda give bureaus this degree of influence over a voting body? This article reports on experiments that demonstrate that a monopoly on the agenda puts the agenda monopolist in a position to bargain with a voting body, without being able to impose his or her most preferred alternative on that body. This evidence supports theories of bureaucratic-legislative bargaining at the expense of theories of bureaucratic imposition.

The orthodox perspective on public administration, developed during the Progressive era, held that efficient, accountable execution of government policy could be best secured through hierarchical, specialized bureaus staffed by professional experts. Woodrow Wilson argued that the efficiency of Continental bureaucracy could be replicated without the taint of "domineering, illiberal officialism" by requiring of public servants a "steady, hearty allegiance to the policy of the government they serve" (Wilson, 1887, p. 14).

Although the notion of accountability through hierarchy and rules has become an integral part of the "managerial orthodoxy" of public administration (see Warwick, 1975, pp. 69-71), academics of various persuasions have spent much of the last half century casting doubts on the effectiveness of this principle. Sociologists such as Gouldner (1954) have demonstrated that an aversion to hierarchy on the part of subordinates can lead to cycles of increasing resistance and rigidity, followed by futile attempts on the part of superiors to regain control by a more strenuous use of authority and regulations, ultimately leading to more resistance. Political scientists have observed that "bureaucratic politics" is more a process of bargaining and gamesmanship among players than a process of imposition down a clear hierarchical line of authority (Allison, 1971). Bureaucrats may evince "steady, hearty allegiance to the policy of the government they serve," but more than likely that policy is the end result of a bargaining process between legislators and bureaucrats, rather than a clear reflection of public opinion.

Although empirical observers such as Wildavsky (1979), Fenno (1966), and a long list of others refer to a bargaining process between actors, that process has rarely been formally incorporated into bureaucratic models. Niskanen's (1971) economic analysis of bureaucratic behavior is no exception. Niskanen dismisses the bargaining process, arguing instead that bureau chiefs are able to impose their preferences regarding budget and production level through control of the agenda. Subject only to production cost constraints and the legislature's maximum willingness to pay for services, the bureau chief in the Niskanen model is the active decision maker, the legislature the passive acceptor of decisions. The Niskanen model stands the Progressive model of accountability on its head.

This article focuses on Niskanen's assumption about bureaucratic imposition through agenda control. Does bureaucratic agenda control allow the bureau chief to impose his preferences on the legislature as is assumed in the Niskanen model? Our view is that the authority to set agendas does not imply the kind of bureaucratic domination and legislative passivity assumed by Niskanen. On the contrary, the agenda setter is limited by the countervailing bargaining position of non-agenda-setting legislators, which in turn implies the rather indeterminate bargaining game documented by Wildavsky and others. This calls for a

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very different model of legislative-bureaucratic interaction, with policy implications strikingly different from those of Niskanen.

Bureaucratic Imposition through Agenda Control

Interest in bureaucratic agenda control developed from the recognition that, since the New Deal, more and more legislation has been originating with the executive branch of government. Indeed, in many instances, the power of the executive is such that it can do virtually what it likes, subject only to a legislative veto. One example is the Executive Reorganizations Act of 1939, which gave the president the right to reorganize the executive branch, subject to a congressional veto within 60 days. The Department of Defense can make real-estate transactions involving the armed services, subject to a veto by the oversight committee. Cities with weak city councils, rotating mayors, and strong professional city managers are often noted for the extent to which they serve only to ratify or veto the proposals of the manager.

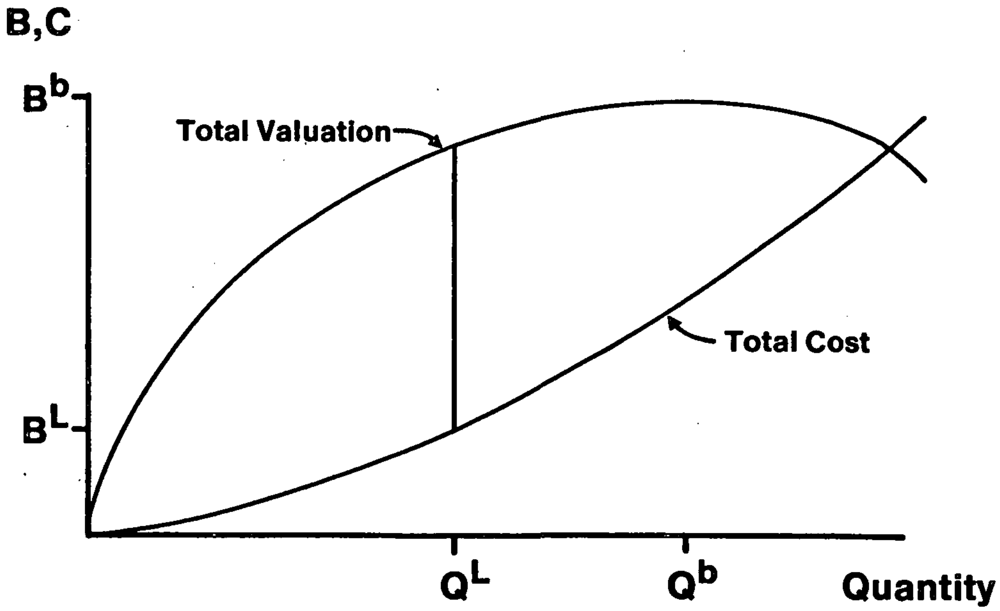
In like fashion, Niskanen created a simplified model in which a passive legislature voted executive proposals up or down. Diagrammatically, the Niskanen model can be represented by Figure 1, which shows both the total cost of producing various levels of a good and the legislature's valuation for the different levels. If the legislature were aware of the cost conditions, it would most prefer to maximize its net benefit by appropriating a budget of B^L for a quantity of Q^L. Note that this level of production corresponds to the maximum difference between the legislature's valuation and the budget outlays.

What would be the most preferred budget and output level for a budget-maximizing agency chief? The valuation curve limits the agency's aspirations in that the legislature would not agree to appropriate a budget larger than its total valuation for a given level of output. Therefore, the bureau would prefer to supply a level of Q^b with a given budget of B^b. According to Niskanen, the bureau achieves this preferred outcome by refusing to propose (B^L, Q^L) as an option to the legislature. Instead, the bureau, through agenda control, exploits its monopoly position by offering its preferred level in a take-it-or-leave-it proposal. The passive legislature, deprived of knowledge of the cost functions, is forced to accept the bureau's suggestion. As a result, production is too large, from an efficiency standpoint, and the requested budget level leaves no consumer surplus for any actor other than the bureau.

In effect, Niskanen assumes a monopolistic bureau chief who achieves perfect price discrimination through a combination of perfect information about the legislature's valuation of the bureau's output and perfect agenda control.

The assumption of bureaucratic imposition through agenda control has been regarded by some economists as Niskanen's "important in-

Figure 1. Preferred Budget Levels for Bureaucrat and Legislature



sight" (Orzechowski, 1977), and has prompted a proliferation of articles on this subject (Borcherding, 1977; Isaac & Plott, 1978; Mackey & Weaver, 1979). Of particular significance for our purposes is the Romer and Rosenthal (1978) extension of the Niskanen model to non-zero status quo points. In the Niskanen analysis, voters are faced with a choice between the bureau's preferred level of provision of the public good or no provision of the public good. What happens when the status quo point shifts from zero to a non-zero position? According to Romer and Rosenthal, the relationship between the median voter's preferred point and the status-quo point determines the size of the bureau's preferred budget. When the status quo coincides with the median voter's preferred point, there is no other alternative preferred by a majority of the committee to that point. In this situation, the agenda setter has no leverage over the electorate. However, as the status quo moves away from the median voter's preferred outcome, the number of alternatives preferred by a majority to the status quo increases. In effect, the agenda setter can impose his preferred outcome from the set of outcomes dominating the status quo. When the status quo is at the point of no provision, the agenda setter has maximum flexibility to enforce his most preferred outcome.

Both the Romer-Rosenthal and Niskanen analyses hinge on the passive voter assumption.[1] But do voting bodies really behave in such a fashion? Niskanen's defense of this assumption is that large legislatures do "not have the incentive or the opportunity to obtain information on the minimum budget necessary to supply this service" (Niskanen, 1971, p. 30). Does agenda control and perfect information mean that the bureaucrat is able to impose his most preferred outcome on a powerless legislature, as Niskanen assumes?

Measuring the Influence of the Agenda Setter

How can we observe whether or not an agenda setter has the influence attributed to him by the models of bureaucratic imposition? In the absence of any agenda control, a majority rule committee will select the Condorcet or majority rule winner if such an alternative exists. The Condorcet winner is defined as the alternative that is preferred by some majority of the committee to any other alternative available to the committee. In general, the conditions for the existence of a Condorcet winner are extreme (Plott, 1967). But when voters have single-peaked preferences in a one-dimensional policy space, the Condorcet winner does exist and is the predicted outcome of the game when *played with an open agenda.* For example, in Figure 2, if voters 1, 2, and 3 constitute a majority-rule committee voting over the policy space from 0 to 110, then alternative 50 will be the Condorcet winner, because it is the median voter's most preferred alternative. Fiorina and Plott (1978) and Berl et al. (1976) have demonstrated the predictive power of the Condorcet winner in experimental settings.[2] Thus, the Condorcet winner stands as a basis for measurement of other effects; agenda control can be said to influence committee decision making to the extent that it produces deviations from the Condorcet winner.

What happens when a voting body does not operate under a completely open agenda? For example, a union membership must vote up or down a single wage-and-benefits package that has been worked out by the union leadership bargaining with employers. Or a school board may offer a tax increase to be voted on by the district electorate; failure to pass the tax proposal would result in the maintenance of the status quo. A committee in the House of Representatives may send a bill to the House protected by a closed rule forbidding amendments from the floor; again failure to pass the committee's bill will result in the maintenance of the status quo. In each situation, there is no guarantee that the agenda setter will offer for the electorate's consideration the alternative that would be the Condorcet winner if the electorate were to operate with an open agenda.

Theoretically, these forms of agenda control can be expected to result in systematic deviations from the Condorcet winner—deviations in the favor of the agenda setter's preferences—because agenda control changes the *dominance relation* among the alternatives facing the committee. For an alternative to dominate any other alternative, there must exist some coalition of the committee which unanimously prefers the first alternative and has the ability to enforce that preferred alternative over the second alternative (Isaac & Plott, 1978). In simple majority rule, the dominance relation is exactly the same as the majority preference relation. However, with an agenda

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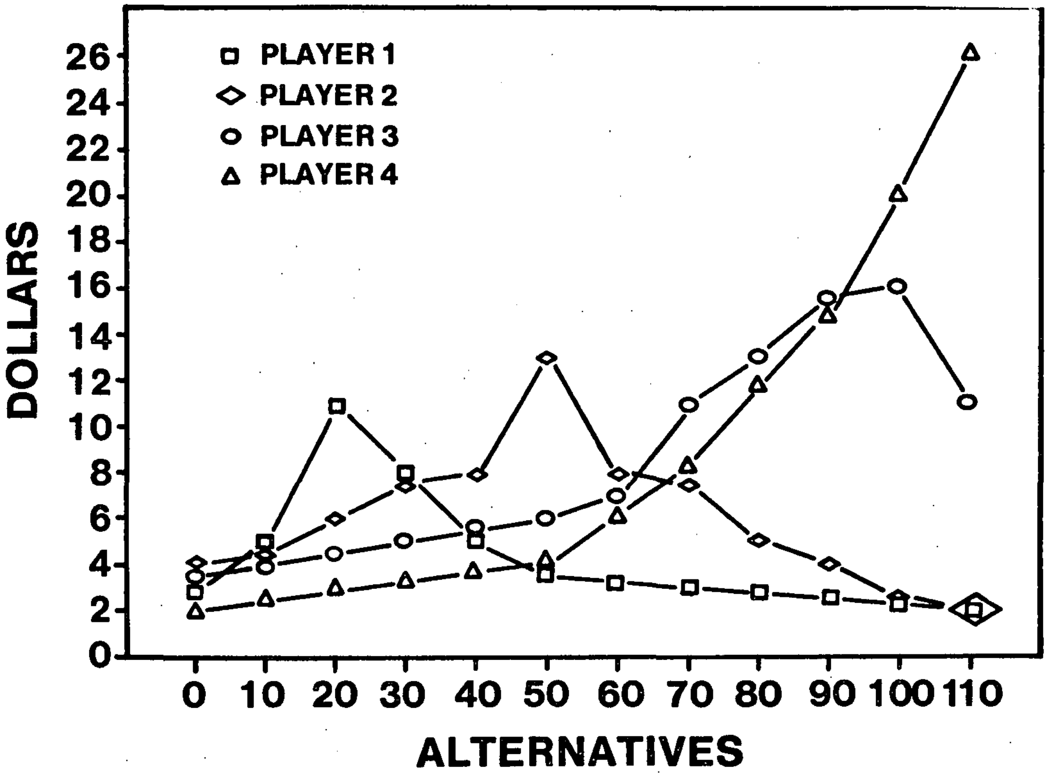
[1] As Romer and Rosenthal put it, they assume individualistic voters who make "voting decisions independently of others," and form no coalitions. Strictly speaking then, their model is applicable only to non-cooperative games and is not tested by the experiments reported in this article, where individuals most emphatically do form coalitions.

[2] The only systematic exception seems to be when, at a very low cost to the winners of a majority rule game, an alternative other than the Condorcet winner is selected which supports the formation of a coalition of the whole; see Eavey and Miller (in press).

Figure 2. Single-Peaked Induced Preferences for Three Voters and Conveners



controller, the status quo may dominate a number of alternatives simply because the convener prefers the status quo to those alternatives. Furthermore, from among the set of alternatives that do dominate the status quo, there is no reason to suppose that the agenda setter will nominate the Condorcet winner over a more preferred alternative.

The basic behavioral assumption in Niskanen can be formally stated as,

> H_B: The agenda setter will successfully impose his most preferred alternative from among the set that dominate the status quo.

We will define the *imposed bureaucratic solution* (IBS) as the alternative from among the set that dominates the status quo that is most preferred by the agenda setter. In Figure 2, assuming zero is the status quo, the set that dominates the status quo is the set from 50 to 90, because both a majority of the committee (voters 2 and 3) and the agenda setter (voter 4) prefer any point in this range to the status quo. (Alternative 90 only weakly dominates the status-quo point because player 2 is exactly indifferent between 90 and zero.) Since the agenda setter most prefers alternative 90 from this set, then 90 is the bureaucratically imposed solution. (The *weak* IBS is the alternative that weakly dominates the status quo; a *strong* IBS strongly dominates the status quo.)

The null hypothesis of no agenda influence can be formally stated as

> H_0: When a Condorcet winning alternative exists, a voting body will select that alternative whether the agenda is open or controlled by a monopolistic agenda setter.

The Bargaining Range

If it does turn out that an agenda setter is able to impose his most preferred alternative from among those that dominate the status quo, then the basic behavioral aspects of Niskanen's model are very reasonable and correct, and Niskanen's recommendations regarding privatization and competition of public service delivery (to eliminate the influence of monopolistic, budget-maximizing bureaucrats) are entirely appropriate.

If agenda control allows imposition of agenda setter's most preferred alternatives, then the lines of accountability envisioned by administrative reformers have been reversed, and bureaucrats are in the driver's seat.

On the other hand, there is an alternative hypothesis that is more in line with the writings of Wildavsky, Fenno, and others who view politics as bargaining between bureaucrats and legislators. This line would suggest that the power to set the agenda merely provides the bureaucrat with bargaining power vis-à-vis the voting body, rather than the power to impose outcomes. In terms of a game suggested in Figure 2, with players 1, 2, and 3 as a voting committee faced by player 4 as agenda setter, the coalition of players 1 and 2 could observe that they are just as necessary for the final decision as the agenda setter. The most effective use of their voting power would be to announce at the beginning of the meeting that they would refuse to vote for any alternative other than the Condorcet winner. Theoretically, they could be just as convincing in this bluff as the convener in his.

The presence of two countervailing veto groups (the voters versus the agenda setter) suggests a bilateral bargaining model. The alternative hypothesis indicated by this line of thinking is as follows:

> H_A: The presence of an agenda setter establishes a *bargaining range* between the Condorcet winning alternative and the agenda setter's most preferred alternative from among the set that dominates the status quo. Bargaining between the agenda setter and the voters will result in some indeterminate alternative within this bargaining range.

In Figure 2, with status quo zero, the bargaining range would be (50-90).

If the alternative hypothesis is correct, then one of Niskanen's key assumptions is incorrect, and the validity of Niskanen's analysis and normative recommendations is called into question. The experiments that follow are designed to determine whether agenda setters can successfully impose their most preferred alternative from among the set that dominates the status quo, or if they must settle for whatever outcome within the bargaining range they can achieve after negotiations with the voting body.

Research Design and Procedures

A total of 42 four-person experiments were run, 18 with discrete alternatives and 24 in the spatial setting. Subjects were recruited either from undergraduate classes at Michigan State University or from an advertisement placed in the MSU student newspaper. In both cases individuals were told that they would be participating in a committee process experiment and were guaranteed at least $2 for participation. During recruitment, individuals were arbitrarily assigned the numbers 1, 2, 3, or 4. Player 4 was designated as the convener, with varying degrees of control over the agenda but no vote.

Players were given a written copy of the instructions, which were also read aloud by the experimenter (see Appendix). Each committee member received only his own compensation chart, whereas the convener was given full information on everyone's preferences; this was done to duplicate the assumptions of agenda-setter information advantage in Niskanen. The agenda setter was not instructed about the use of his knowledge. He was told that the other players had only their own compensation charts and that he was free to use the information as he saw fit, excluding any discussion of actual (cardinal) monetary amounts. Committee members were likewise prohibited from mentioning monetary values, although any ordinal revelation of preferences was allowed. The task of the committee in the discrete experiments was to choose one letter from the set of letters (*A*–*L*). In the spatial experiments, individuals were instructed to select a number from the continuum of numbers (0–110), with 0, 10, 20 . . . corresponding to *A*, *B*, *C*. . . . The convener and the voting members were separated for an initial discussion period of 5-15 minutes, during which the convener was allowed to familiarize himself with the information on his own and other payoff charts. During the same time, the members were asked "to try to come up with some agreement as to their preferred option."

In cases in which the convener had agenda control, the committee's decision was made by majority rule of the three voting members, choosing between the status quo option and the convener's official, written proposal. In cases with an open agenda, the convener had the same information advantages and the committee engaged in the same discussion period, but anyone could propose an option to be voted on against the status quo. In all cases, if the players failed to reach an agreement, the status-quo alternative (either *A* or *D* in the discrete experiments) automatically would become the committee's choice. This option could be enforced unilaterally by the convener or by a majority of the committee voting without the convener.

This basic research design was held constant through the 42 experiments. In order to provide a crucial test of various competing hypotheses, two institutional and two preference variables were changed. The institutional variables were the status-quo or reversion option (either *A* or *D*) and the degree of agenda control held by the convener. The motivation for manipulating these conditions and the preference conditions will be discussed below; in each case, manipulation of these independent variables provided additional information about the various solution concepts being tested. Thus, the experimental design allowed a static test (of various solution concepts in any given run of the experiment) and dynamic tests (of the effects of *changes* in institutional or preference variables).

Part 1. Discrete Policy Alternatives

By soliciting four volunteers, telling them the rules of the game, and promising them a payoff determined by the committee's choice over letters *A* through *L* as in Table 1, it is possible to test the effectiveness of the bargaining range against the imposed bureaucratic solution. According to Niskanen, the convener should be able to impose upon the committee his preferred alternative *J* simply by threatening to propose the status quo option *A*. If the results of the experiments are systematically less than *J* but greater than *F*, we can conclude that monopoly agenda power alone is sufficient to guarantee an imposed bureaucratic solution.

Status Quo A and the Niskanen Analysis. A series of four experiments were run with the instructions given in the Appendix, status quo at *A*, and the preferences listed in Table 1. The convener or agenda setter was allowed to make as many proposals as he wanted, or he could refuse to propose alternatives for consideration. However, the convener was not a voting member of the committee and therefore required a majority of the committee (two or more members) for ratification of his proposed alternatives.

The dynamics of these experiments were fairly uniform: the convener would press for an advantageous alternative with the support of player 3, players 1 and 2 would express a willingness to accept the status-quo alternative if not presented with a more favorable proposal, and a compromise solution would be ratified. But one particular experiment in which subjects were recruited from a junior-level banking class highlights the intricacies of our four-person game.

Players were randomly assigned to roles (players 1 and 2 were female and players 3 and 4 were male). Player 4, as agenda setter, was given full information on committee preferences. Instructions were read. Committee members were given 5 to 15 minutes to discuss among themselves their own preferences and recommendations while the convener was in another room looking over his charts. As alternatives were eliminated from consideration, the committee discussion focused on five acceptable alternatives: *E, F, D, G,* and *H*.

Player 4 was invited back into the room, and player 2 announced the committee's preferences for *E* or *F*. The following is a partial transcript of what transpired:

Player 4 claims that *I* and *F* are not acceptable and announces, "I am going to tell you straight out that I have sitting here right in front of me, a copy of a compensation chart" for each committee member.

> Player 3: Well, that stinks.
>
> Player 4: According to the rules the meeting can be convened (closed) at any time. By the convener. What you see in front of me here is an order which

Table 1. Compensation Chart for Committee Members and Agenda Setter

	Players			
Alternatives	1	2	3	4[a]
A	$ 2.75	$ 4.00	$ 3.50	$ 2.00
B	5.00	4.50	4.00	2.50
C	11.00	6.00	4.50	3.00
D	8.00	7.50	5.00	3.25
E	5.00	8.00	5.50	3.75
F	3.50	13.00	6.00	4.00
G	3.25	8.00	7.00	6.00
H	3.00	7.50	11.00	8.25
I	2.75	5.00	13.00	11.75
J	2.50	4.00	15.50	15.00
K	2.25	2.50	16.00	20.00
L	2.00	2.00	11.00	26.00

[a]Agenda setter.

> would close the meeting, if what I propose is not accepted.

(The convener is attempting a Schelling strategy by threatening to enforce the status quo alternative if his first proposal is not accepted by the committee.)

The convener then proposes option *L*.

> Player 2: I am really against *L*, and I would just as soon close the meeting because I don't care.

This initiates a discussion between player 3, who supports the convener's proposal, and player 2, who is ready and willing to close the meeting. The interaction between players convinces the convener that he lacks needed votes for option *L*. Reneging on his original take-it-or-leave-it position, the convener offers the committee another option.

> Player 4: I'll tell you what, then. I've listed some other proposals that I think we might be able to find amicable. *L* was my first choice. How does *K* look to you?

Players 1 and 2 immediately reply that *K* is no better than *L*. The convener asks about *J*, and player 2 says, "*J* looks about as good as closing the meeting."

At this point, the convener spends some time trying to split the coalition between players 1 and 2 by referring to his information and pointing out that player 2 would have done a lot better than #1 with the committee's original proposals, letters *E* and *F*. He tries to cast her as a Machiavellian manipulator unworthy of player 1's support; an effort to which player 3 contributes.

> Player 3: She sure did. She really pushed me into *E*.

Player 2 responds by pointing out that the convener was all bluff, having threatened to close the meeting if the committee failed to support his first proposal. In turn, the convener claims that he did not go ahead and close the meeting as he threatened because he is a nice guy just trying to find a fair alternative for everyone, "but you guys are going to have to trust me."

> Player 1: Well, let me hear your proposal. I'm not into this trusting business. We don't really have to do anything.
>
> Player 4: You don't have to do anything. You can always walk out with proposal *A*.
>
> Player 1: That's right.
>
> Player 2: That's right.

The tables now have turned, with players 1 and 2 threatening player 4 with the status quo. The coalition between players 1 and 2 is as strong as ever, despite player 4's efforts.

The convener then makes another attempt to split the coalition between 1 and 2 by making the claim that player 2 would do better with the status quo alternative *A* than anyone else on the committee, again painting her as a Machiavellian manipulator.

> Player 4: There is going to be one person in this experiment who wouldn't mind getting *A*. And in this case, it happens that the person who wouldn't mind getting *A* and who would walk off all right, would be you (in reference to player 2).

Player 4 continues, saying that there is a "chasm" between the value of *A* for her and the value of *A* for everyone else. This is a blatant misrepresentation of the information that is available only to him; i.e., the cardinal values of the members' payoffs. He then says that he will try to find some fair alternative that will give #1 "a nice piece of the action also. See, I don't think that there is a reason why anyone has to walk off (with nothing)."

Player 4 then proposes alternative *I* as a fair solution. Player 1 points out that she is indifferent between *I* and *A*, thus undercutting player 4's claim to impartial expertise. Player 1 then takes the initiative and proposes letter *E*, to which the convener reiterates his refusal to accept *E*. In some desperation, player 2 asks the experimenter if there is any way to vote on an alternative without the convener. When the experimenter repeats the rules, player 4 laughs malevolently. At this point, player 3 sees that the breakdown between the convener and players 1 and 2 is getting very close to complete and proposes alternative *H* as a compromise solution. The convener spends a great deal of time resisting *H*: "There's got to be something better than *H*." However, as the only way to resolve the deadlock, player 4 eventually proposes letter *H*, which the committee ratifies.

The models of bureaucratic imposition assume that the convener has monopoly power over the agenda, exclusive and complete information on committee preferences, and the willingness to use this information to achieve his most preferred alternative. Certainly, in the example detailed above, the convener was eager and willing to use all available resources to secure the highest possible payoff, including the dispensing of false or misleading information. But still he was unable to reach his most preferred alternative. Why? Because the convener lacks one very crucial resource: the votes needed to ratify his proposed alternative. The committee has the power to act as a veto block simply because they supply the votes.

And this is what happened in all four experiments. *H* was picked three times and *G* once.[3] Note that the imposed bureaucratic solution was never chosen. The game is not one of bureaucratic imposition, but rather a bargaining game in which the bureaucrat is apt to fall short of complete success, owing to the existence of a countervailing blocking coalition of voters one and two.

Variation I–Separating the Core from the Bargaining Range. Although the hypothesized bargaining range includes as extreme points the preferred outcomes of the two pivotal players (player 2 as median voter and player 4 as agenda setter), it was predicted that a majority of the outcomes would lie between those extremes. True to our prediction, the outcomes selected in the previous experiments were always between the median voter's preferred alternative and the im-

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[3] Four pilot experiments, with similar preferences to the first set of discrete experiments, resulted in *H* twice, *I* once, and *E* once.

Table 2. Revised Compensation Chart for Committee Members and Agenda Setter

Alternatives	Players 1	2	3	4[a]
A	$ 4.75	$ 4.00	$ 3.50	$ 2.00
B	5.00	4.50	4.00	2.50
C	11.00	6.00	4.50	3.00
D	8.00	7.50	5.00	3.25
E	5.00	8.50	6.50	3.75
F	3.50	13.00	5.50	4.00
G	3.25	8.00	7.00	6.00
H	3.00	7.50	11.00	8.25
I	2.75	5.00	13.00	11.75
J	2.50	4.00	15.50	15.00
K	3.15	2.50	16.00	20.00
L	4.00	2.00	11.00	26.00

[a]Agenda setter.

posed bureaucratic solution. This corresponds with the notion of the bargaining range introduced earlier. However, it also corresponds with the theoretic notion of the core.

The core is defined as the set of undominated alternatives. The letters *F*-*J* constitute the core of the four-person game given by the preferences in Table 1, the status quo equal to *A*, and the decision rule defined as majority rule subject to player 4's agenda setting. Alternative *H*, for instance, is undominated because no coalition that prefers some other alternative can enforce it. Players 1 and 2 prefer alternative *F* to *H* but cannot propose it because they lack agenda power. Players 3 and 4 prefer *J* to *H*, but cannot get another vote for *J*. No coalition can upset *H*, or any other alternative in the bargaining range. So the question arises, was *H* selected as a compromise solution between two veto groups (i.e., because it was part of the bargaining range) or because it was undominated in the game-theoretic sense (i.e., because it was part of the core)?

These two explanations can be distinguished by separating the core from the bargaining range. We did this by modifying slightly the preferences to provide a critical test between the core and the bargaining range (Table 2). What is the core of this new game? Notice that alternatives *F* through *J* are dominated by a three-person coalition consisting of the convener and committee members 1 and 3 around either alternative *K* or alternative *L*. Alternatives *K* and *L* are dominated by alternative *A* (the status quo), which is dominated by alternative *F*. Every alternative is dominated by some other alternative except alternative *E*. Alternative *E*, therefore, is the core of the new game. However, the median voter's preferred alternative is still option *F*, and the convener's preferences are identical to the previous experiments. The preferences for our pivotal players over the bargaining range (*F*-*J*) are still in opposition, even though this range is no longer the core of the game. If alternative *H* was selected because it was part of the core, then it should no longer be selected when the core is at *E*. If alternative *H* was selected as the outcome of a compromise between the outside agenda setter and the median voter, then it should also be the committee selection when it is no longer the core.

The experimental evidence supports the bargaining range over the core. In five experiments with alternative *E* as the core and option *A* the status quo, the core was never selected as the outcome of a game (see Table 3). Three of the outcomes (options *G, H,* and *H*) were within the bargaining range, one outcome was located at the preferred point of the agenda setter, and one outcome, alternative *L*, was outside the bargaining range entirely. Although the dynamics of the game were unaffected by the separation of the core and bargaining set, the new preference configurations allowed a smart convener to exploit the intransitivities in preferences. For example, the convener could propose alternative *H* as a compromise between himself and player 2, and then mobilize a coalition of players 1 and 3 around alternative *L* (preferred by them to alternative *H*), despite the fact that *L* is dominated by the status quo *A*.

Variation II–Altering the Status Quo. Romer and Rosenthal argue that the position of the status-quo point vis-à-vis the committee has an important effect on the agenda setter's power by limiting or expanding the range of alternatives dominating the status quo. When the status quo is at zero or rather, a level of no provision (as in the earlier experiments), the convener has maximum flexibility in that the number of alternatives dominating the status quo is larger than at any other point. The convener is then in the position of being able to impose a highly favorable alternative. However, as the status quo approaches the median voter's most preferred outcome, the number of alternatives dominating the status quo decreases substantially. Given the preferences in Table 1 and a status quo point of option *D*, only alternatives *F, G,* and *H* dominate the status quo (*D* is weakly dominated by *H*). The best that an outside agenda setter can hope for is to impose alternative *H* on the committee, whereas in the earlier experiments the convener could hope to do substantially better than *H* by imposing his most preferred alternative *J*.

A total of nine experiments were run which replicated both sets of earlier experiments with the new status-quo point *D*. The weak, bureaucratically imposed solution is option *H*, the point at which the median voter is just indifferent between the status quo and the convener-preferred alternative. Again, notice that the bargaining range contracts from (*F-J*) to (*F-H*). Four experiments were run with the core equal to the bargaining range, while five experiments were run with a new core of option *E*.

The results indicate that a shift in the status quo marginally affects the bargaining range by constraining the power of the agenda setter (see Table 3). With alternative *A* as the status quo, all but two cases resulted in a committee choice of alternative *H* or better, from the convener's perspective. With the status quo option *D*, only four of the results were at alternative *H* or higher. Thus, moving the status quo toward the median voter's preferred outcome seems to limit the convener's ability to "win" the bargaining game.

It appears that power over the agenda has put the convener at the bargaining table without sufficient influence to impose a most-preferred outcome. But do the experiments really indicate the influence of agenda control, or is the bias away from the Condorcet winning alternative toward the nonvoting player 4's preferred outcome simply the result of some small group effect such as an informal grant to the convener by the committee of an equal voice or "vote" in the collective deliberations?[4] The following experiments were designed to determine whether player 4's influence results from an informal vote or from agenda power.

Part 2: A Spatial Policy Setting

Preferences were graphically induced in individuals over a continuum of numbers ranging from 0 to 110 (Figure 2). Notice that these preferences correspond almost exactly to the preferences listed in Table 1 of the discrete experiments, with minor deviations incurred to smooth out the preference curves. The number 0 corresponds to alternative *A* in the discrete experiments, the number 10 to alternative *B*, and so on. By arranging alternatives over a continuum, players have the option of bargaining for values between discrete points. The spatial setting allows us to increase the bargaining range and look for subtle, minor effects without significantly altering the design of the original experiment.

A total of 24 experiments were run in the spatial setting. Unlike the discrete experiments, the status quo was unchanged at number 0 (corresponding to alternative *A*). However, changes were made in

Table 3. Experimental Results: Discrete Policy Alternatives

	Preferences	
Status Quo	Table 1	Table 2
A		
Core	F-J	E
WIBS[a]	J	J
Results	G,H,H,H	G,H,H,J,L
D		
Core	F-H	E
WIBS	H	H
Results	G,G,I,K	E,G,G,H,H

[a]Weak imposed bureaucratic solution. The point at which the median voter is just indifferent to the status quo. It is thus the most favorable outcome that the agenda setter could hope to impose on the committee.

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[4] We would like to thank Charles Plott for this suggestion, and for his support and timely comments throughout the research presented in this article.

Table 4. Experimental Results: Spatial Policy Setting

Institutions	Voter 3's Preferences: As in Figure 2	Voter 3's Preferences: Identical to Voter 2
Strong agenda control by player 4		
WIBS[a]	1) 90	2) 90
Results	60,65,70,80	45,50,70,70
Weak agenda control by Player 4 (unlimited proposals)		
WIBS	3) 90	4) 90
Results	55,70,70,100	55,55,60,65
Open agenda		
Condorcet winner	5) 50	6) 50
Results	50,50,50,65	43,47,50,50

[a]Weak imposed bureaucratic solution.

the institutions (the locus and extent of agenda control) and in the composition of the committee's preferences. We ran four experiments that corresponded, institutionally and with regard to preferences, to the first experiments in the discrete setting; that is, only the convener could make formal proposals, and he could make any number of proposals. The results are reported in Table 4, cell 3. The chosen outcomes were equivalent to two choices of *H*, a point midway between alternatives *F* and *G*, and one choice at *K*. In the latter instance, the convener utilized both his monopoly over information and agenda control to achieve an outcome outside the predicted bargaining range. By convincing the committee that 100 was the best outcome from the standpoint of aggregate group benefit, the convener maximized his individual payoff without misrepresenting information. The total group payoff with option 100 was approximately $41, compared to approximately $31 for alternative 70. Overall, the effect of agenda control was apparent, given that the original committee recommendations to the convener centered around the Condorcet winning alternative (specifically, they were 50, 50, 55, and 60).

We then changed the preferences in such a way that player 3's preferences were exactly like those of player 2, with a peak at 50. The core of this four-voter game is exactly equal to alternative 50, which is the first choice of players 2 and 3, and between the first choice of players 1 and 4. If the favorable outcomes achieved by player four resulted from an unofficial "vote" granted him by the other members of the committee, then the results should indicate this by shifting to 50 since the Condorcet winner of the four-voter game is exactly equal to 50. If the favorable outcomes were due to the convener's ability to "impose" a Niskanen and Romer-Rosenthal outcome, then the bias should not be affected in the least, since the Romer-Rosenthal prediction does not change with the change in player 3's preference. If, however, the bias in outcomes toward player 4's preferred outcome was because agenda control gave him some bargaining influence, then the bias should decrease as the bureaucrat's natural ally on the committee disappears and sides exactly with the median voter.

The results (Table 4, cell 4) are consistent with the final hypothesis. The bias in favor of player 4's preferred alternative was sharply weakened, *even though the imposed bureaucratic solution was unchanged.* If the agenda monopolist is able to impose his most preferred solution from among the set of alternatives that dominate the status quo, then player 4 should have been able to get a result in the range of 70 or better, as in the earlier experiments. However, with the absence of a natural ally on the committee, the convener's ability to impose a solution unattractive to the committee diminished, so that he never got a result above 65.

This result seems to demonstrate that the bias for the bargaining game is in fact the existence of two countervailing blocking coalitions. Player 4 can block anything to the left of 90 simply by not proposing anything to the left of 90; but players 1 and 2 can block anything to the right of 50 simply by not voting for anything to the right of 50. Yet some agreement must be reached or both sides will be hurt. By placing two players in the median voter position (and none in the player 3 role), the credibility of the legislative veto power is increased. Either median voter is sufficient to block the agenda setter's more preferred alternatives, and the two players can support each other in vetoing player four's proposals.

At the same time, we should not underplay the ability of the convener to alter predicted outcomes. If player 4 had been simply exercising an informal "vote," then his influence over the committee should have disappeared as the core of the voting game converged on 50 with the change in player 3's preferences. Yet this was not the case. In our experiments, the final committee outcomes were never below 55, even though the three members of the committee unanimously preferred 50 to anything higher. Player 4 could not have achieved this result simply as another voter in a four-person majority rule game with two voters most preferring 50. He was able to achieve it only because agenda control made him a one-person blocking coalition who must give consent to the final decision. The results are therefore a striking disconfirmation of the bureaucratic imposition hypothesis, but also a striking confirmation of the ability of a convener to alter committee decisions through agenda control and bargaining.

Eliminating Bureaucratic Agenda Control. There are three possible explanations for the consistent ability of the convener to persuade committee members to deviate from the Condorcet winning alternative: agenda control, the convener's monopoly over information, or an artifact of the experimental design in which the convener is dramatically brought into the room to do "something" after the committee has had a chance to discuss their preferences among themselves. If the experimental results are attributable to the subject's surmises about what the experimenter "wants" from this experimental design, then the convener is obviously put in a unique and strategic position.

To control for the possibility that something other than agenda control was behind the convener's success in the previous experiments, a new set of experiments were run in which the convener retained his monopoly over information and his dramatic entrance in the middle of the experiment, but was deprived of his right to propose alternatives for consideration. In effect, the locus of control shifted from the convener to the committee. Eight experiments were run with this new institution, four with player 3's original preferences and four with player 3 having identical preferences to player 2. The results, displayed in cells 5 and 6 of Table 4, support the median voter hypothesis in the absence of monopolistic agenda control. The median voter's preferred outcome (number 50) was chosen in five experiments. In the three experiments in which the Condorcet winning alternative was not the final outcome of the meeting, a more advantageous alternative for player 1 was selected twice, and only once did the committee slightly favor the nonvoting player 4. In the one instance in which the committee biased their selection towards player 4, player 3's preferences were compatible with those of the nonvoting player.

As the results indicate, when player 4 was no longer in control of the agenda, his influence over the committee disappeared completely. Although players 2 and 3 would occasionally sacrifice their most preferred outcome in order to provide player 1 with an improved payoff, thus assuring committee unanimity, the players exhibited no desire to come to some unanimous agreement with player 4. This is convincing evidence that the previous bias in experiment results in favor of player 4 resulted from his monopoly control over the agenda, and not his control over information, his unofficial "vote" as a fourth committee member, a flaw in the design, or altruism on the part of the committee towards player 4.

Strong Agenda Control. One possible objection to the previous experiments is that they allowed the agenda monopolist to introduce multiple proposals to the committee, thus weakening his ability to impose an outcome by allowing the committee to vote down any number of proposals with the assurance that the convener could always introduce another. On the other hand, if the agenda setter were allowed only one proposal, he could in effect blackmail the committee by limiting their choice to the convener's proposal or the status quo. This sort of situation epitomizes the take-it-or-leave-it analysis of Niskanen. If the bureaucrat fails to impose his most preferred alternative on the committee in this situation, then his lack of success in games with multiple proposals would not be surprising.

It should be noted that, from a practical standpoint, few agenda monopolists legally can make only one proposal. Congress considers as negotiable any budget proposal passed down from the executive branch. A proposal from the union leadership for a strike settlement always can be rejected by the membership (although a cost may be incurred). Even the strongest form of agenda control, that of a school board with regard to its tax referenda, is not truly a take-it-or-leave-it proposition. Opponents to a particular tax measure can and often do urge voters to turn down one proposal in order to force the school board to introduce a more palatable measure the next time, perhaps in a matter of months. It is hard to imagine a real-world agenda monopolist being limited to only one proposal.

Furthermore, previous experiments by Isaac and Plott (1978) on this very issue found no difference in outcomes whether the convener was allowed one or several proposals. But in spite of the evidence to the contrary and in order to provide the strongest possible case for the hypothesis of bureaucratic imposition, we designed a series of experiments to test the effectiveness of the convener when limited to only one proposal. The committee, in turn, could vote the proposal up or down by approving either the convener's recommended alternative or the status quo point 0. Unlike the earlier experiments, the balloting was done in secret, and the result of the vote was announced to the committee by the experiments (see Appendix for complete instructions).

Eight experiments followed the strict Niskanen interpretation, four with identical preferences for players 2 and 3 and four with opposing preferences. The results, reported in Table 4, cells 1 and 2, did not provide any new support for the hypothesis of bureaucratic agenda imposition. All but one committee outcome was located within the bargaining range. The only extreme outcome favored the least advantaged member of the committee (player 1) and not the agenda controller. Player 4, far from being able to impose the Romer-Rosenthal outcome of 90, seemed even less able to impose his wishes than in the previous experiments; that is, his bargaining position was weakened under the new institution.

Why was the position of the convener undermined when the number of proposals was limited to one? The ability of the convener to impose his most preferred outcome was affected by two factors: the near-perfect symmetry of the bargaining positions of player 4 and the committee as two opposed blocking coalitions, and the increased risk aversion on the part of player 4. The one-proposal rule improved player 4's ability to bluff as it simultaneously increased the negative consequences of making a bluff: if the first proposal failed, there would be no chance for a second. Our conveners expressed a fear of making a proposal that would not succeed, leaving themselves and the committee with the status quo, which was, after all, worse for him than for anyone else. And once again, the committees reinforced the agenda setter's fear of making a proposal that would be voted down by uttering quite believable threats that they in fact would vote down a proposal that was a long way from alternative 50. Of course, the closer the agenda setter's proposal is to 90, the more believable are the threats since the committee has little to lose near this outcome. The agenda monopolists clearly saw this game as a trade-off between the amount of their payoff and the certainty of receiving it. The agenda setters believed that the voters were capable of giving up small amounts to punish the agenda setters for proposing a "selfish" alternative. And, of course, the more selfish the proposal, the smaller the amount of money the committee members would need to sacrifice. Particularly when players 2 and 3 had identical preferences, a committee threat to veto anything above 50 was taken seriously into account by the agenda setters. This fact was reflected in the outcomes of the bargaining process.

Conclusion

The fundamental conclusion to be reached from these experiments is that an agent outside a legislative body with control over the agenda does have influence over legislature's decisions, but not to the extent suggested by the theories of bureaucratic imposition. In our experiments, the agenda setter was able to produce an immediate and sustained bias away from the Condorcet winner, a bias that disappeared when agenda control was removed. At the same time, the agenda setter was limited because he could not unilaterally impose a most preferred outcome. The coalition of players 1 and 2 constituted a blocking coalition, and *player 2 had to be won over.* Player 2's awareness of this fact guaranteed that the outcome would be determined by a bargaining game between the median voter and the convener. Agenda control may put the agenda setter at the bargaining table, but it does not guarantee imposition of his most preferred alternative.

The implications of this fact are clear. *Theories of bureaucratic agenda imposition should be replaced with theories of bureaucratic bargaining.* Emphasis needs to be placed on the key factors structuring the relationship between the bureau and the voting body. Which characteristics improve the position of the voting body vis-à-vis the bureau? Unanimity is one such characteristic, as suggested by our experiments. In those situations in which players 2 and 3 were in agreement, thus denying player 4 a natural ally on the committee, the bargaining position of player 4 was greatly weakened.

From a policy perspective, it is worth noting that William Niskanen is now a member of the President's Council of Economic Advisers, and that bureaucrats are very much a symbol of all that is under attack in the current administration's program of privatization and cut-back in (non-defense) public services. To the extent that this program is based on economic theories attributing budgetary growth to self-serving bureaucrats, the justification for the administration's program may be extremely weak. Even if bureaucrats are the ones who make the obvious and telling agenda proposals to legislatures, that is not sufficient to absolve legislatures of the blame for any excessive governmental growth. Budget-maximizing bureaucrats may have a predisposition toward budgetary growth, but it is doubtful whether they can impose their wishes on voting bodies, especially when their control over the agenda is much weaker than that embodied in the experiments in this article. Contracting for services with private suppliers may or may not be nice, but it is certainly not a panacea for all the budgetary problems of the eighties. The more politically realistic explanations for budgetary growth must look at the bargaining behavior of legislatures, as well as that of bureaucrats, and the institutions that they create to structure the bargaining process. Research of this kind is now being conducted by Fiorina (1977), Fiorina and Noll (1978), Shepsle (1978), Shepsle and Weingast (1981) and others. Only with a better understanding of legislators and their incentives as members of oversight and appropriations committees will we have a complete understanding of both sides in the game of bureaucratic bargaining.

Appendix
Instructions for Participants
Spatial Setting with Weak Agenda Control

1. We would like to have you participate in a committee process experiment. The purpose of the experiment is to help us understand certain technical aspects of the generally complex ways in which committees operate. The instructions are simple. If you follow them carefully and make good decisions, you might earn a considerable amount of money. You will be paid in cash.

2. All you have to do is to attend a committee meeting and for this participation you will be paid. The purpose of the meeting is to choose a number from the set of numbers zero through 110. [In discrete setting, this is "the set of letters A-L."] Only one number will be chosen and the payment you receive for participation depends *entirely* upon which one it is. For example, on the enclosed compensation graph, you can read *up* from the number 30 to the line graph, and then *over* to the vertical axis to discover the amount you will be paid if the number 30 is the committee's choice. The amount for other numbers can be discovered similarly. The compensation tables may differ among individuals. This means that the patterns of preferences differ and the monetary amounts may not be comparable. The option that would result in the highest payoff to you may not result in the highest payoff to someone else. You should decide what choice you want the committee to make and do whatever you wish within the confines of the rules to get things to go your way. The experimenters, however, are not primarily concerned with whether or how you participate as long as you stay within the confines of the rules. Under no circumstances may you mention anything quantitative about your compensation. You are free, if you wish, to indicate which ones you like best, etc., but you cannot mention anything about the actual monetary amounts. Under no circumstances may you mention anything about activities which might involve you and other participants after the experiment (i.e., no deals to split up afterwards or no physical threats).

3. **Procedures.** The following parliamentary procedures must be followed. Player 4 has been designated as a convener. The convener is *not* a member of the committee and therefore *cannot* vote on any of the proposals. But, the option chosen by the committee will be the one *proposed* by the convener and *ratified* by a majority of the committee (two or more people). Any failure of the committee to act automatically results in a committee choice of option 0.

The committee will have a discussion period lasting a minimum of 5 minutes and a maximum of 15 minutes in which the convener is not present. During this time, the committee, operating under majority rule, should try to come to some agreement as to their preferred option.

[In Weak Agenda Control Experiments]

At the conclusion of the discussion period, the convener will address the committee. Committee members are free to make suggestions to the convener. *The convener may propose any option he wants, or he can refuse to propose options if he so desires.* He proposes options in writing by completing and signing the forms provided. Any proposal by the convener automatically cancels any previous, unratified proposals. A proposal is ratified if it is signed by a majority of the committee; i.e., to be ratified, a proposal must be signed by the convener and approved by at least two committee members. A proposal once ratified is final, and all participants will be paid accordingly. A proposal that is not ratified and is then cancelled by another proposal may be reproposed again and again.

4. Terminating the meeting. The meeting can terminate in any of three ways:

i) If some option is proposed and ratified, the meeting is terminated and the committee members are paid on the basis of the committee choice.

ii) If a motion to adjourn is approved by a majority of the committee, the meeting is terminated. *Any* committee member is free to introduce such a motion at any time. The motion must be submitted in writing and signed by a majority of the committee. If the meeting adjourns without a choice, then option 0 is automatically designated as the choice and you will be paid accordingly.

iii) The convener has the right to adjourn the meeting at any time. He does so simply by submitting the announcement in writing to the experimenter. Again, if the meeting adjourns without a choice, then option 0 is automatically designated as the choice and you will be paid accordingly.

[In Open Agenda Experiments]

At the conclusion of the discussion period, the convener will formally open the decision period for the committee. Open discussion may continue; the convener is free to make suggestions to the committee. At any time during the decision phase, any committee member may make any formal proposal he likes. A committee member proposes options in writing by completing and signing the forms provided. Any proposal by a committee member automatically cancels any previous, unratified proposals by that committee member. A proposal is ratified if it is signed by a majority of the committee; i.e., to be ratified, a proposal must be made by a committee member and approved by at least one other committee member. A proposal once ratified is final, and all participants will be paid accordingly.

4. Terminating the meeting. The meeting can terminate in either of two ways:

i) If some option is proposed and ratified, the meeting is terminated, and the committee members are paid on the basis of the committee choice.

ii) If a motion to adjourn is approved by a majority of the committee, the meeting is terminated. *Any* committee member is free to introduce such a motion at any time. The motion must be submitted in writing and signed by a majority of the committee. If the meeting adjourns without a choice, then option 0 is automatically designated as the choice and you will be paid accordingly.

[In Strong Agenda Control Experiments]

At the conclusion of the discussion period, the convener will address the committee. Committee members are free to make any suggestions to the convener. Discussion will be open for as long as the convener wishes. Whenever he chooses to do so, however, the convener may officially propose an option. *The convener may propose any option he wants.* He proposes an option in writing by completing and signing the form provided. The proposal is official, and discussion ends when the convener hands the proposal to the experimenter. At that time, each of the three committee members casts a secret ballot for either the status quo alternative 0 or for the alternative proposed by the convener. The ballots will be counted by the experimenter. If the convener's proposal gets two or more votes, then it wins. Otherwise, the status quo alternative wins. The winning alternative is final and all participants will be paid accordingly.

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The New Institutionalism: Organizational Factors in Political Life

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Contemporary theories of politics tend to portray politics as a reflection of society, political phenomena as the aggregate consequences of individual behavior, action as the result of choices based on calculated self-interest, history as efficient in reaching unique and appropriate outcomes, and decision making and the allocation of resources as the central foci of political life. Some recent theoretical thought in political science, however, blends elements of these theoretical styles into an older concern with institutions. This new institutionalism emphasizes the relative autonomy of political institutions, possibilities for inefficiency in history, and the importance of symbolic action to an understanding of politics. Such ideas have a reasonable empirical basis, but they are not characterized by powerful theoretical forms. Some directions for theoretical research may, however, be identified in institutionalist conceptions of political order.

In most contemporary theories of politics, traditional political institutions, such as the legislature, the legal system, and the state, as well as traditional economic institutions, such as the firm, have receded in importance from the position they held in the earlier theories of political scientists such as J.W. Burgess or W.W. Willoughby, economists such as Thorstein Veblen or John R. Commons, and sociologists such as Max Weber. From a behavioral point of view, formally organized social institutions have come to be portrayed simply as arenas within which political behavior, driven by more fundamental factors, occurs. From a normative point of view, ideas that embedded morality in institutions, such as law or bureaucracy, and that emphasized citizenship as a foundation for personal identity, have given way to ideas of moral individualism and an emphasis on conflicting interests.

In recent years, however, a new institutionalism has appeared in political science. It is far from coherent or consistent; it is not completely legitimate; but neither can it be entirely ignored. This resurgence of concern with institutions is a cumulative consequence of the modern transformation of social institutions and persistent commentary from observers of them. Social, political, and economic institutions have become larger, considerably more complex and resourceful, and *prima facie* more important to collective life. Most of the major actors in modern economic and political systems are formal organizations, and the institutions of law and bureaucracy occupy a dominant role in contemporary life.

Attention to political institutions has increased in the literature on legislatures (Shepsle & Weingast, 1983), budgets (Padgett, 1981), public policymaking (Ashford, 1977; Scharpf, 1977), local government (Kjellberg, 1975), and political elites (Robins, 1976). It is manifest in studies of the origin of the state (Wright, 1977) and the development of national administrative capacity (Skowronek, 1982), in analyses of the breakdown of democratic regimes (Potter, 1979), and in discussions of corporatism (Berger, 1981; Olsen, 1981; Schmitter & Lehmbruch, 1979). It is reflected in the Marxist rediscovery of the state as a problem in political economy (Jessop, 1977) and of the importance of organizational factors for understanding that role (Therborn, 1980). It is present in studies of formal organizations and particularly in studies of the place of such organizations in the implementation of public policy (Hanf & Scharpf, 1978). It is visible in attempts to link the study of the state to natural science (Masters, 1983) and to the humanities

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(Geertz, 1980), as well as in a renewed interest in making historical-comparative studies of the state (Evans, Rueschemeyer, & Skocpol, 1983; Hayward & Berki, 1979; Krasner, in press).

In this article we examine some aspects of these developments and their implications for developing a theoretical understanding of how political life is organized. We approach the task from the perspective of students of formal organizations. The argument, however, extends beyond organization theory to a more general view of the place of institutions in politics and the possibilities for a political theory that is attentive to them.

Theoretical Styles of Contemporary Political Science

Although the concept of institution has never disappeared from theoretical political science, the basic vision that has characterized theories of politics since about 1950 is (a) *contextual*, inclined to see politics as an integral part of society, less inclined to differentiate the polity from the rest of society; (b) *reductionist*, inclined to see political phenomena as the aggregate consequences of individual behavior, less inclined to ascribe the outcomes of politics to organizational structures and rules of appropriate behavior; (c) *utilitarian*, inclined to see action as the product of calculated self-interest, less inclined to see political actors as responding to obligations and duties; (d) *functionalist*, inclined to see history as an efficient mechanism for reaching uniquely appropriate equilibria, less concerned with the possibilities for maladaptation and non-uniqueness in historical development; and (e) *instrumentalist*, inclined to define decision making and the allocation of resources as the central concerns of political life, less attentive to the ways in which political life is organized around the development of meaning through symbols, rituals, and ceremonies.

Politics as Subordinate to Exogenous Forces: Contextualism

Historically, political scientists and political philosophers have tended to treat political institutions, particularly the state, as independent factors, important to the ordering and understanding of collective life (Heller, 1933). Modern political scientists, with few exceptions, have not. The state has lost its position of centrality in the discipline; interest in comprehensive forms of political organization has declined; political events are defined more as epiphenomena than as actions necessary to an understanding of society; politics mirrors its context (Easton, 1968).

The most conspicuous contextual factor cited in recent writing is the social class structure. The social stratification of a modern society with its associated distribution of wealth and income has obvious major effects on political events. Class differences translate into political differences with great reliability across time and across cultures; differences in the organization and ideology of social class seem to lead to predictable differences in political organization and institutions (Tilly, 1978). Other analyses at the same level of aggregation make the structure and process of politics a function of physical environment, geography, and climate; of ethnicity, language, and culture; of economic conditions and development; or of demography, technology, ideology, or religion. Plausible arguments which make political life a derivative of one or more of these broad contextual forces have been developed, and it is not hard to find empirical data to support the arguments. Although there are a number of relatively precise contextual theories, the major theoretical significance of these ideas from the present point of view is less the specific forms of the theories than their general inclination to see the causal links between society and polity as running from the former to the latter, rather than the other way around. It is assumed that class, geography, climate, ethnicity, language, culture, economic conditions, demography, technology, ideology, and religion all affect politics but are not significantly affected by politics.

The Macro Consequences of Micro Behavior: Reductionism

Historically, political theory has treated political institutions as determining, ordering, or modifying individual motives, and as acting autonomously in terms of institutional needs. In contrast, substantial elements of modern theoretical work in political science assume that political phenomena are best understood as the aggregate consequences of behavior comprehensible at the individual or group level.

Such theories depend on two presumptions. The first presumption is that a political system consists of a number (often a large number) of elementary actors. Human behavior at the level of these elementary actors may be seen as conscious, calculated, and flexible, or as unconscious, habitual, and rigid. In either case, the preferences and powers of the actors are exogenous to the political system, depending on their positions in the social and economic system. The second presumption is that collective behavior is best understood as stemming from the (possibly intricate) interweaving of behavior understandable at a lower level of aggregation. Discovering, or deducing, the collective consequences may be difficult, even impossible; but the central faith is that outcomes at the collective level depend only on the intricacies of the interactions among the individual actors, that concepts suggesting autonomous behavior at the aggregate level are certainly superfluous and probably deleterious.

Within such a perspective, for example, the behavior of an organization is the consequence of the interlocking choices by individuals and subunits, each acting in terms of expectations and preferences manifested at those levels (Niskanen, 1971). The behavior of a market is the consequence of the interlocking choices by individuals and firms, each acting in terms of a set of expectations and preferences manifested at those levels (Stigler, 1952). It is not necessary that the micro processes involve choice, of course. Aggregate behavior in a group can be defined as the consequence of the interlocking of trial-and-error learning occurring at the individual level (Lave & March, 1975). Or the aggregate behavior of an industry can be defined as the consequence of the interlocking of standard operating procedures and accounting rules followed at the level of the individual firm (Nelson & Winter, 1982).

There is nothing intrinsic to a perspective that emphasizes the macro consequences of micro actions which requires that the elementary units be individuals. All that is required is that the behavior of a more comprehensive system be decomposable to elementary behaviors explicable at a less comprehensive level. In practice, however, in most of the social sciences, the actions of individual human beings are considered to determine the flow of events in a larger social system. Outcomes at the system level are thought to be determined by the interactions of individuals acting consistently in terms of the axioms of individual behavior, whatever they may be. Thus, we make assumptions about individual consumers to understand markets, about voters to understand politics, and about bureaucrats to understand bureaucracies.

The two best-specified theories of aggregate behavior in the social sciences, the economic theory of markets and the ecological theory of environmental competition, exemplify the modern style. Consider the theory of markets. Within this theory we find individual consumers, each attempting to make purchases at the best possible prices considering his or her own preferences and alternatives, and individual producers, each attempting to make production and pricing decisions that result in the best possible return considering his or her own preferences and alternatives. The behavior of the market is assumed to be understandable as a consequence of these individual actors making choices that, in aggregate, fit together into market phenomena. Consider similarly the ecological theory of environmental competition. Within this theory we find individual species, each adapting to an environment through survival, mutation, and reproduction. Selection and changes in population distributions within the environment are assumed to be understandable as consequences of the actions of individual actors that, in combination with the actions of others and the potential capacity of the environment, produce a distribution of types.

Action as the Making of Calculated Decisions: Utilitarianism

Historically, political science has emphasized the ways in which political behavior was embedded in an institutional structure of rules, norms, expectations, and traditions that severely limited the free play of individual will and calculation (Wolin, 1960). In contrast, modern political science has, for the most part, described political events as the consequence of calculated decisions. Not just in political science, but throughout modern theoretical work in the social sciences, the preeminent vision of human behavior is a vision of choice. Life is characterized as deliberate decision making.

The details of the choice metaphor vary from one treatment to another, but the characteristic form is one that assumes choices stem from two guesses about the future. The first is a guess about the uncertain future consequences of possible current action. Decision theorists recognize that human limitations may restrict the precision of the estimates, that the estimates may be biased, and that the information on which the estimates are based may be costly; but information about probable consequences is assumed to be important to a choice. From this assumption comes an emphasis on the power of information and expertise (Crozier, 1964) and the importance of reliable and unbiased information sources (Nisbet & Ross, 1980). Although numerous psychological experiments have indicated that the guesses of human subjects are biased (Kahneman, Slovic, & Tversky, 1982), it has not been easy to formulate alternatives to the simple notion that the guesses of experienced humans are, on average, accurate. As a result, most theories of choice present decisions as being, on average, sensible. In their political versions, choice theories assume that, on average, voters vote intelligently with respect to their interests; legislators organize sensible coalitions, given their interests; and nation states voluntarily enter alliances that, on average, improve their positions.

The second guess on which intentional, anticipatory choice is based is a guess about a decision maker's uncertain future preferences for possible future outcomes. In any theory of deliberate choice, action depends on the decision maker's values. Since the consequences of interest are to be realized in the future, it is necessary to anticipate not only what will happen but how the decision maker will feel about those outcomes when they are experienced (March, 1978). The compexities of the second guess are largely ignored by theories of choice. In their standard forms, the theories assume that preferences are stable, thus that current preferences are good predictors of future preferences; that preferences are unambiguous and consistent, thus that a choice will be clearly indicated, given the first guess; and that preferences are exogenous, thus that whatever process generates preferences, it precedes choice and is independent of the choice process. In one of the best-developed forms of choice theories, these assumptions about preferences are taken as axioms, and preferences are discovered not by asking decision makers to report them but by defining a "revealed preference" function that satisfies the axioms and is consistent with choices made by a decision maker (Luce & Raiffa, 1957). Although the empirical existence of consistent revealed preferences has been the subject of considerable debate (Becker & Stigler, 1977; Kahneman, Slovic, & Tversky, 1982), the theoretical idea forms the basis of extensive analytical development and empirical exploration.

The Efficiency of History: Functionalism

Historically, political theory has been ambivalent about the efficiency of history. Like other social scientists, students of political development have been inclined to accept an idea of progress, the more or less inexorable historical movement toward some more "advanced" level. At the same time, political histories have often emphasized the unique significance of a particular sequence of events or choices, the impact of a particular campaign strategy or speech, or the particular tactics of international negotiation. In modern usage, the terminology of progress has been largely replaced by a terminology of survival, but for the most part, in contemporary theoretical political science, institutions and behavior are thought to evolve through some form of efficient historical process.

An efficient historical process, in these terms, is one that moves rapidly to a unique solution, conditional on current environmental conditions, thus independent of the historical path. The equilibrium may involve a stochastically stable distribution or a fixed point, but we require a solution that is achieved relatively rapidly and is independent of the details of historical events leading to it. The assumption of historical efficiency is a standard, although usually not explicit, assumption of much of modern social science. Economic theories of markets and ecological theories of competition, for example, are concerned with the characteristics of an equilibrium, if one exists. They are used to predict differences (e.g., in markets, organizational structures, population, technologies) that will be observed, at equilibrium, in different environments. Similarly, some postwar theories of political parties see party orientation and organization as equilibrium solutions to problems of survival in a competitive political environment (Downs, 1957). The assumption of historical efficiency makes such theories largely indifferent to the behavioral reality of the micro processes that are assumed. For example, competition can be assumed to eliminate action that is inconsistent with the logic of survival. Examples include theories of market equilibria, such as those found in recent ideas of efficient capital markets (Sharpe, 1970); theories of organizational structure, such as those found in recent ideas of industrial organization (Williamson, 1978); and theories of political parties, such as those found in ideas of political economy (Olson, 1965).

History cannot be guaranteed to be efficient. An equilibrium may not exist. Even if there is an equilibrium, historical processes can easily be slow enough relative to the rate of change in the environment that the equilibrium of the process is unlikely to be achieved before the environment, and thus the equilibrium, changes. By assuming quickness, theories of political behavior avoid focusing on transient phenomena that might be less predictable and more subject to effects from the details of the processes involved. For example, when it is predicted that political parties will come to identical positions in an environment of single-peaked voter preferences, it is assumed that party adjustment will be much more rapid than will be changes in voter preferences. Efficiency also requires that the equilibrium be unique and achievable. Processes with multiple equilibria are, of course, easily specified and frequently observed. What makes them unattractive is not their rarity, but their intractability and the indeterminacy of their outcomes. It is no accident that the most common principle of theories in the social sciences is the optimization principle, and that one of the greatest concerns in such theories is showing that a process has a unique optimum that is guaranteed to be achieved.

The Primacy of Outcomes: Instrumentalism

Historically, theories of political institutions portrayed political decision making primarily as a process for developing a sense of purpose, direction, identity, and belonging. Politics was a vehicle for educating citizens and improving cultural values. Although there are exceptions, the modern perspective in political science has generally given primacy to outcomes and either ignored symbolic actions or seen symbols as part of manipulative efforts to control outcomes, rather than the other way around.

Modern polities are as replete with symbols, ritual, ceremony, and myth as the societies more familiar to anthropological tradition. Politicians announce public support for positions they fail to defend in private (Edelman, 1964). Legislators vote for legislation while remaining indifferent to its implementation (Pressman & Wildavsky, 1973). Administrators solicit public participation in decision making in order to secure public support for policies to which they are already committed. Chief executives advocate reorganization of the public bureaucracy, announce plans for making reorganizations, and regularly abandon the plans (March & Olsen, 1983). Information is gathered, policy alternatives are defined, and cost-benefit analyses are pursued, but they seem more intended to reassure observers of the appropriateness of actions being taken than to influence the actions (Feldman & March, 1981).

In modern discussions of politics, these symbolic actions are characteristically portrayed as strategic moves by self-conscious political actors. Rituals and ceremonies are defined as window-dressing for the real political processes, or as instruments by which the clever and the powerful exploit the naive and the weak. The hiring of experts lends legitimacy to policies (Meyer & Rowan, 1977); associating unpopular moves with popular symbols is reassuring (Edelman, 1964). Control over symbols is a basis of power, like control over other resources (Pfeffer, 1981a); and the use of symbols is part of a struggle over political outcomes (Cohen, 1974).

Institutionalist Perspectives

The new institutionalism is not peculiar to political science. Renewed interest in institutions is characteristic of recent trends in economics, which has discovered law, contracts, hierarchies, standard operating procedures, professional codes, and social norms (Akerlof, 1980). It is also seen in anthropology and sociology, although non-institutionalist visions never succeeded in those fields to the extent that they did in political science and economics. Nor are the institutionalist ideas entirely new. By labeling the collection of ideas "the new institutionalism," we mean to note the fact that there was indeed an "old institutionalism," that cycles in ideas have brought us back to considerations that typified earlier forms of theory in political science. We do not mean to suggest, however, that the new and the old are identical. It would probably be more accurate to describe recent thinking as blending elements of an old institutionalism into the non-institutionalist styles of recent theories of politics.

This new institutionalism can be presented and discussed as an epistemological perspective of profound importance to understanding social science, but for our purposes, it is more useful to define it in terms of a narrow collection of challenges to contemporary theoretical thinking in political science, a small set of relatively technical ideas of primary interest to professional students of political life. The ideas deemphasize the dependence of the polity on society in favor of an interdependence between relatively autonomous social and political institutions; they deemphasize the simple primacy of micro processes and efficient histories in favor of relatively complex processes and historical inefficiency; they deemphasize metaphors of choice and allocative outcomes in favor of other logics of action and the centrality of meaning and symbolic action. The ideas are not all mutually consistent. Indeed, some of them seem mutually inconsistent. For example, ideas based on the assumption that large institutional structures (e.g., organizations, legislatures, states) can be portrayed as rationally coherent autonomous actors are uneasy companions for ideas suggesting that political action is inadequately described in terms of rationality and choice.

The Causal Position of Political Institutions

Without denying the importance of both the social context of politics and the motives of individual actors, the new institutionalism insists on a more autonomous role for political institutions. The state is not only affected by society but also affects it (Katzenstein, 1978; Krasner, 1978; Nordlinger, 1981; Skocpol, 1979; Stephan, 1978). Political democracy depends not only on economic and social conditions but also on the design of political institutions. The bureaucratic agency, the legislative committee, and the appellate court are arenas for contending social forces, but they are also collections of standard operating procedures and structures that define and defend interests. They are political actors in their own right.

The argument that institutions can be treated as political actors is a claim of institutional coherence and autonomy. The claim of coherence is necessary in order to treat institutions as decision makers. From such a point of view, the issue is whether we wish to picture the state (or some other political institution) as making choice on the basis of some *collective* interest or intention (e.g., preferences, goals, purposes), alternatives, and expectations (Levi, 1981). There is no necessary answer to the question unless we impose one. Whether it makes pragmatic theoretical sense to impute interests, expectations, and the other paraphernalia of coherent intelligence to an institution is neither more nor less problematic, a priori, than whether it makes sense to impute them to an individual (Kahneman, 1982; March & Shapira, 1982). The pragmatic answer appears to be that the coherence of institutions varies but is sometimes substantial enough to justify viewing a collectivity as acting coherently.

The claim of autonomy is necessary to establish that political institutions are more than simple mirrors of social forces. Empirical observations seem to indicate that processes internal to political institutions, although possibly triggered by external events, affect the flow of history. Programs adopted as a simple political compromise by a legislature become endowed with separate meaning and force by having an agency established to deal with them (Skocpol & Finegold, 1982). The establishment of public policies, or competition among bureaucrats or legislators, activates and organizes otherwise quiescent identities and social cleavages (Olsen & Saetren, 1980; Tilly, 1978). Policy experts within the political system develop and shape the understanding of policy issues and alternatives (Heclo, 1974).

Such phenomena are not routinely accommodated by modern political theory, which makes political outcomes a function of three primary factors: the distribution of preferences (interests) among political actors, the distribution of resources (powers), and the constraints imposed by the rules of the game (constitutions). Each of these is treated as exogenous to the political system. That is, preferences are developed within a society and transmitted through socialization, resources are distributed among political actors by some broad social processes, and rules of the game are either stable or change by a revolutionary intervention exogenous to ordinary political activities.

The idea that preferences are produced and changed by a process that is exogenous to the processes of choice is fundamental to modern decision theory. In the "revealed preference" version of the theory, preferences must be stable in order for the theory to be testable. In other versions, preferences can change, but choice itself does not produce a change in preferences. Conventional theories of markets, for example, picture advertising and experience as providing information about alternatives and their properties, not as affecting tastes. Similarly, conventional theories of politics assume that a voter's exposure to and choice of a candidate do not change that voter's preferences for various attributes that a candidate might possess, although they may change a voter's beliefs about which candidates possess which attributes. The new institutionalism, in company with most research on preferences, argues that preferences and meanings develop in politics, as in the rest of life, through a combination of education, indoctrination, and experience. They are neither stable nor exogenous (Cohen & Axelrod, in press). If political preferences are molded through political experiences, or by political institutions, it is awkward to have a theory that presumes preferences are exogenous to the political process. And if preferences are not exogenous to the political process, it is awkward to picture the political system as strictly dependent on the society associated with it.

The contrast between the two kinds of notions is found most starkly in theories of political leadership. One classic idea of political leadership emphasizes the creation of winning political coalitions among participants with given demands (March, 1970). The leadership role is that of a broker: providing information, identifying possible coalitions, and facilitating side-payments and the development of logrolls. Such a view of leadership is implicit in the theory of the political process that has been developed in political science in recent decades. A second conception of leadership emphasizes the transformation of preferences, both those of the leader and those of the followers (Burns, 1978; Selznick, 1957). Leaders interact with other leaders and are coopted into new beliefs and commitments. The leadership role is that of an educator, stimulating and accepting changing worldviews, redefining meanings, stimulating commitments. Such a view is more conspicuous in the ideas of the new institutionalism.

The distribution of political resources is also partly determined endogenously. Political institutions affect the distribution of resources, which in turn affects the power of political actors, and thereby affects political institutions. Wealth, social standing, reputation for power, knowledge of alternatives, and attention are not easily described as exogenous to the political process and political institutions. Holding office provides participation rights and alters the distribution of power and access (Egeberg, 1981; Laegreid & Olsen, 1978). The policy alternatives of leaders are not defined completely by exogenous forces, but are shaped by existing administrative agencies (Skocpol, 1980; Skocpol & Finegold, 1982; Skowronek, 1982). The outcomes of the political process modify reputations for power, which in turn modify political outcomes (Enderud, 1976; March, 1966).

Finally, the third exogenous factor in conventional theories of politics, the rules of the game, is not really exogenous either. Constitutions, laws, contracts, and customary rules of politics make many potential actions or considerations illegitimate or unnoticed; some alternatives are excluded from the agenda before politics begins (Bachrach & Baratz, 1962); but these constraints are not imposed full-blown by an external social system; they develop within the context of political institutions. Public agencies create rules and have them sanctioned by politicians (Eckhoff & Jacobsen, 1960), and revolutionary changes are initiated and pursued by military bureaucrats (Trimberger, 1978).

The Causal Complexity of Political History

Theories of politics tend to assume a relatively uncomplicated intermeshing among the elementary units of a political system. There may be many individuals, groups, or classes involved, but they are relatively undifferentiated and their interactions are relatively simple. Empirical observations of political systems, on the other hand, often stress the institutional complexity of modern states (Ashford, 1977; Scharpf, 1977) and identify a rather complicated intertwining of institutions, individuals, and events. Alternatives are not automatically provided to a decision maker; they have to be found. Search for alternatives occurs in an organized context in which problems are not only looking for solutions, but solutions are looking for problems. Information about the consequences of alternatives is generated and communicated through organized institutions, so expectations depend on the structure of linkages within the system, as well as the ways in which biases and counter-biases cumulate (Simon, 1957a, b). Guesses about future preferences are developed within institutions dedicated to defining and modifying values and the meanings of actions (Cyert & March, 1963; March & Olsen, 1976). There are many such institutions, some nested within others, with multiple, overlapping connections (Long, 1958). National political systems fit into international political systems and are composed of numerous subsystems, some of which extend beyond national boundaries.

If this complexity is not decomposable analytically into smaller systems or susceptible to some relatively simple aggregation techniques, the theoretical problems of understanding social history are not easily accommodated within contemporary theoretical styles. For example, it may be rash to assume that errors in expectations have a normal distribution with a mean of zero. The allocation of attention may be critical to the flow of events. The responsiveness of the political system to environmental pressures may, at least in the short run, depend on the amount of slack in the system, and on the ways in which accounting numbers are produced and fudged. The system may not come close to trying to resolve conflict but simply attend sequentially to the demands placed on it (Cyert & March, 1963). Learning may be superstitious, and fallacious rules of inference may persist for long periods (Nisbet & Ross, 1980). At the limit, the connections between problems and solutions may be less dominated by a logic of causal linkages between means and ends than by the less problematic temporal linkages of simultaneity (Cohen, March, & Olsen, 1972).

Theories of collective behavior most commonly simplify the potential morass of collective complexity by one of two classic routes. The first is statistical aggregation. In its usual guise, aggregation assumes that the factors affecting outcomes can be divided into two groups, one systematic and the other random. Thus, for example, we might assume that in a population of voters there are many factors affecting electoral choice. Some of those factors (e.g., income) have impacts on the vote that are strong and consistent across individuals. Other factors (e.g., specific policy issues) have impacts that are weaker or less consistent or less well understood. If we assume the latter factors can be treated as noise, that is, that they are independent, randomly distributed variables, the systematic factors will be clear in the aggregate results. In this way, conventional assumptions of aggregation impose a statistical order on the results.

The second classical simplification is the assumption of historical efficiency. Although the argument is usually associated with theories of natural selection and best specified in modern theories of population biology, the basic idea of historical efficiency is implicit in many modern theories. Regardless of the complexity or apparent anomalies of human behavior, historical processes are assumed to eliminate rules for behavior that are not solutions to an appropriate joint optimization problem. Thus, a prediction based on solving the optimization problem will correctly predict behavior, regardless of whether the actors involved formulate or solve that problem explicitly (Friedman, 1953). For example, we might predict the outcome of a complicated political negotiation by assuming that the actors are each acting rationally on the basis of complete information about each other and the world, even though we recognize that such assumptions are quite false as a description of individual behavior.

Students of institutions suggest alternative theoretical simplifications for understanding complex political systems, most commonly the assumption of a political structure. By a political structure we mean a collection of institutions, rules of behavior, norms, roles, physical arrangements, buildings, and archives that are relatively invariant in the face of turnover of individuals and relatively resilient to the idiosyncratic preferences and expectations of individuals. In contrast to theories that assume action is choice based on individual values and expectations, theories of political structure assume action is the fulfillment of duties and obligations. The difference is important. In a choice metaphor, we assume that political actors consult personal preferences and subjective expectations, then select actions that are as consistent as possible with those preferences and expectations. In a duty metaphor, we assume that political actors associate certain actions with certain situations by rules of appropriateness. What is appropriate for a particular person in a particular situation is defined by the political and social system and transmitted through socialization.

Political structure simplifies a complex world for the individuals in it. It does not necessarily, however, simplify the problems of the political theorist. The complex intermeshing of rule-driven behavior may be just as difficult to unravel as the complex intermeshing of preference-driven behavior. As a result, there has long been a tendency to combine ideas of political structure with ideas of historical efficiency. If individual behavior is driven by rules within a political structure, then it is possible to imagine that historical experience accumulates over generations of individual experience. The information about that experience is encoded in institutional rules (Nelson & Winter, 1982). This argument is a familiar one to political discourse. It has been a part of conservative doctrine for hundreds of years, forming a basis for defending both traditional rules of behavior and the existing political order.

The advantage to treating behavior as rule driven, in addition to its apparent consistency with numerous observations, is not that it is possible thereby to "save" a belief in historical efficiency; rather, it is that it leads more naturally than does treating behavior as optimization to an examination of the specific ways in which history is encoded into rules, and thus to making the idea of historical efficiency more attentive to the possible limiting conditions for efficiency, and more likely to generate interesting predictions about multiple equilibria or long time paths. In fact, the assumption of efficiency becomes mostly a matter of faith if the joint optimization problem cannot be specified or solved by the observer, or if it is impossible to identify the precise mechanisms by which historical experience is transformed into current action. Unless the process is specified, it is impossible to examine either the likelihood that a particular equilibrium will be achieved or how long it will take.

Politics as an Interpretation of Life

A conception of politics as decision making is at least as old as Plato and Aristotle. It is reflected in the language and concerns of political thought, from the earliest political philosophers through Bentham to Merriam and Lasswell. Who gets what and how? For the most part, contemporary theory in political science considers politics and political behavior in such instrumental terms. The intent of actions is found in their outcomes, and the organizing principle of a political system is the allocation of scarce resources in the face of conflict of interest. Thus, action is choice, choice is made in terms of expectations about its consequences, meanings are organized to affect choices, and symbols are curtains that obscure the real politics, or artifacts of an effort to make decisions.

Parts of the new institutionalism are challenges to this primacy of outcomes. These challenges echo another ancient theme of political thought, the idea that politics creates and confirms interpretations of life. Through politics, individuals develop themselves, their communities, and the public good. In this view, participation in civic life is the highest form of activity for a civilized person. The ideas find post-Hellenistic voices in J.S. Mill, Pateman (1970), and Lafferty (1981). Politics is regarded as education, as a place for discovering, elaborating, and expressing meanings, establishing shared (or opposing) conceptions of experience, values, and the nature of existence. It is symbolic, not in the recent sense of symbols as devices of the powerful for confusing the weak, but more in the sense of symbols as the instruments of interpretive order.

The primary source of the institutionalist challenge is empirical. Observers of processes of decision making regularly discern features that are hard to relate to an outcome-oriented conception of collective choice. The pleasures are often in the process. Potential participants seem to care as much for the right to participate as for the fact of participation; participants recall features of the process more easily and vividly than they do its outcomes; heated argument leads to decisions without concern about their implementation; information relevant to a decision is requested but not considered; authority is demanded but not exercised (Feldman & March, 1981; March & Olsen, 1976). These observations are often reported as anomalies, as symptoms of some kind of perversity in the systems that were observed, paradoxical. The appearance of paradox, however, is a product of our theoretical presumption that the main point of a decision-making process is a decision. For many purposes, that presumption may be misleading. The processes of politics may be more central than their outcomes.

Politics and governance are important social rituals. In older worlds in which the major causal force producing historical experience was the will of the gods, social rituals were organized around ceremonies by which that will was discovered and influenced. Most contemporary developed societies, being somewhat more secular in their conceptions of causality, believe that experience is produced by a combination of natural laws and intentional human action. In these societies, therefore, social and political rituals are organized around the consultation of expertise and the making of decisions (Olsen, 1970). The procedures of decision that we observe are reflections and reminders of this modern, secular conception of the social order. They are signals and symbols of the appropriateness of events, not in the sense that what happened needs to be viewed as desirable or pleasant, but in the sense that what happened can be viewed as having occurred in the way things happen (Feldman & March, 1981). The usual term is "legitimate"; but legitimacy may denote something narrower than is intended, for what rituals seek to establish is not only the moral virtue of events but also their necessity.

Theoretical Research and Political Institutions

Human actions, social contexts, and institutions work upon each other in complicated ways, and these complex, interactive processes of action and the formation of meaning are important to political life. Institutions seem to be neither neutral reflections of exogenous environmental forces nor neutral arenas for the performances of individuals driven by exogenous preferences and expectations. As a result, contemporary political theory is probably overly sanguine about the possibilities for a theory of politics that ignores political institutions.

For the most part, however, the relevant theoretical work remains to be done. It is interesting to suggest that political institutions and the society are interdependent, but that statement needs to find a richer theoretical expression. It is appropriate to observe that political institutions can be treated as actors in much the same way we treat individuals as actors, but we need more detailed demonstrations of the usefulness of doing so. There is good sense in noting that history is not necessarily efficient, but it would be of greater help if we were able to show the specific ways by which specific history-dependent processes lead to outcomes that are either non-unique or long delayed under some conditions. It is plausible to argue that politics is filled with behavior that is difficult to fit into a utilitarian model, but the plausibility would be augmented if we could describe an alternative model. And it is provocative to note the importance of symbols, ritual, ceremony, and myth in political life, but we cannot sustain the provocation without a clearer specification of how theories of politics are affected by such a vision.

Moving from the subtle judgments of empirical knowledge to an appropriate theoretical formulation is no easier in the analysis of politics than it is elsewhere. It requires not only further empirical studies but also theoretical research. By theoretical research we mean primarily the development of ideas, concepts, and models based on empirical observations and relevant to a behavioral understanding and prescriptive ordering of political life. The objective is not impossible. Thirty years ago, empirical students of organizations made two major criticisms of the existing theory of organizational decision making. The first criticism was that the theory made extraordinary time and information demands on organizations (March & Simon, 1958; Simon, 1957a, b). Information and time were treated as freely available resources. To ask that all consequences of all alternatives be known precisely seemed unreasonable in the face of empirical evidence that organizations considered only a small number of alternatives, examined only a small number of consequences related to only a subset of organizational goals, and made relatively imprecise estimates.

The second criticism was that the theory assumed that all participants in an organization shared the same goals, or if they did not, that conflict among them could be readily managed through the terms of some prior agreement (Cyert & March, 1963; March, 1962). In the case of a political organization, the agreement was a coalition contract, or constitution, by which all members of a coalition or polity agreed to be bound to the policies specified through bargaining or legislation. Thus, the familiar distinction between "politics" and "administration." In the case of an economic organization, the agreement was an employment contract by which employees, in return for the payment of wages, agreed to act as though they had the same goals as the owner or other legitimate policy maker. Empirical studies seemed to indicate that conflict was endemic in organizations and that it tended to be interminable rather than settled by prior agreements.

These criticisms began to have serious impact on formal theories of organized action when they were translated into useful theoretical statements through the development of information economics and theories of agency. Such theories consider information as a scarce resource subject to strategic action in a world populated by self-interested rational actors. Ideas drawn from organizational studies of bounded rationality and internal conflict permeate modern economic theory in the form of discussions of moral hazard, asymmetric information, agency, signalling, and optimal information strategies (Hirshleifer & Riley, 1979). Most students of organizations would argue that these theories are also incomplete, but it is clear that the earlier empirical criticisms have reformed theoretical thinking.

The new institutionalism would benefit from similar theoretical development if it could be accomplished. Like the early observations about bounded rationality and internal conflict, observations about the importance of institutions have generally taken the form of criticism of existing theoretical ideas rather than the delineation of an alternative set of precise theoretical concepts. Developing a comprehensive theoretical structure for institutional thinking is, of course, a prodigious and pretentious task, not one that will be undertaken here. We can, however, identify a few ideas associated with the new institutionalism that might warrant theoretical attention.

Institutional Conceptions of Order

Institutional thinking emphasizes the part played by institutional structures in imposing elements of order on a potentially inchoate world. Traditional political theory involved considerable attention to the order produced by political contracts and reflected in constitutions, laws, and other stable rules, or by a community of moral obligation, often inspired and buttressed by religious dogma (Berki, 1979; Waterstone, 1966). For the most part, modern political theory eschews such concerns and focuses on aggregation and historical efficiency superimposed on two other kinds of order: the order imposed by reason and the order imposed by competition and coercion. Reason is recognized in ideas of rationality and intentional action; it finds institutional expression in the hierarchical organization of means and ends (and thus in formally planned institutions). Competition and coercion are recognized in ideas of conflict of interest, power, bargaining, survival, and war; they find institutional expression in elections and policymaking. Theoretical research relevant to the new institutionalism would involve elaborating additional notions of political order. We believe it is possible to identify at least six such conceptions on which a modest amount of theoretical work might yield rewards.

Historical Order. The concept of historical order implicit in contemporary theory emphasizes the efficiency of historical processes, the ways in which history moves quickly and inexorably to a unique outcome, normally in some sense an optimum. An institutional theory would specify how historical processes are affected by specific characteristics of political institutions, and it would provide greater theoretical understanding of the inefficiencies of history, i.e., historical processes that do not have equilibria, take extended periods of time, lead to non-unique equilibria, or result in unique but suboptimal outcomes. Theoretical attention to the inefficiencies of history involves a greater concern for the ways in which institutions learn from their experience (Etheredge, 1976) and the possibilities that learning will produce adjustments that are slower or faster than are appropriate or are misguided. It involves trying to specify the conditions under which the sequential branches of history turn back upon each other and the conditions under which they diverge. It involves characterizing the role of standard operating procedures, professions, and expertise in storing and recalling history.

Temporal Order. In most theories of action, we assume things are ordered by their consequential connections. Means are linked to appropriate ends; causes are linked to effects they produce; consequences are linked to actions that lead to them and to preferences they affect; solutions are linked to problems they solve. Such concepts of order underlie theories of choice. Deviations from consequential order are viewed as interesting aberrations, disturbances of a system otherwise held together by the way wanting something leads to doing something connected to the want, and doing something leads to consequences related to the intention. Temporal order provides an alternative in which linkages are less consequential than temporal. Things are connected by virtue of their simultaneous presence or arrival. In a culture with a strong sense of monthly or yearly cycles or of birth cohorts, we should not be overly surprised by temporal order. In many human situations the most easily identified property of objects or events is the time subscripts associated with them. Thus, students of time allocation in organizations have observed the ways in which attention to problems seems to be determined as much by the time of their arrival as by assessments of their importance. A classic form of temporal order is found in queuing theory, although most discussions of queuing are embedded in a consequential structure in which queues are either indistinguishable or distinguishable only by their processing times.

Endogenous Order. Much of contemporary theory emphasizes the way order is imposed on political institutions by an external environment. From this perspective, for example, power within a political system is determined by possession of resources in the environment, interests are determined by position in the external world, and coherence within an institution is assured by the exigencies of existence. Thus, order is effectively exogenous to the institution and does not depend on properties of the institution or processes within it. Students of institutions have suggested a number of ways in which internal institutional processes affect things like the power distribution, the distribution of preferences, or the management of control. As a result, they invite theoretical development of models appropriate for understanding the ways in which interests and preferences develop within the context of institutional action, the ways reputations for power evolve as a result of the outcomes of politics, the ways in which the process of controlling purposive organizations produces unanticipated consequences, and the ways in which the course of decision making within political systems systematically, and endogenously, results in illusions of success and failure.

Normative Order. It is a commonplace observation in empirical social science that behavior is constrained and dictated by cultural dicta and social norms. Although self-interest undoubtedly permeates politics, action is often based more on discovering the normatively appropriate behavior than on calculating the return expected from alternative choices. As a result, political behavior, like other behavior, can be described in terms of duties, obligations, roles, and rules. Such a description has not, however, been translated into any very compelling theoretical form. Some efforts have been made to rationalize normative rules, such as altruism (Kurz, 1978) and reciprocity (Axelrod, 1980), or to specify the conditions for their evolution (Axelrod & Hamilton, 1981; Trivers, 1971). From an institutionalist perspective, such efforts are exemplary, but they tend to limit attention to the comparative statics of individual norms. A broader theoretical examination of normative order would consider the relations among norms, the significance of ambiguity and inconsistency in norms, and the time path of the transformation of normative structures. A theoretical understanding of such conventional norms as those surrounding trust and legitimacy seems likely to be particularly germane to political analysis.

Demographic Order. It is tempting for students of politics, as for students of other human endeavor, to find order defined in terms of the logic of their particular domain of interest. Thus, students of legislatures imagine that a legislature is best understood in terms of lawmaking, and students of courts imagine that a court is best understood in terms of adjudicating. Alternatively, a human institution can be studied and interpreted as the cross-section of the lives of the people involved. The idea that collective behavior can be understood as a mosaic of private lives links contemporary theoretical thought to similar ideas among qualitative students of human behavior and novelists (Krieger, 1983). A focus on institutional demography combines such a vision of organized life with attention to a property of individual lives that is itself a product of the institutional structure—the individual career (March & March, 1978; Pfeffer, 1981b). The theoretical requirements include useful concepts of the ways in which organizations adapt through turnover, institutions are driven by their cohort structures, and the pursuit of careers and professional standards dictates the flow of events.

Symbolic Order. Students of formal organizations have called attention to the ordering force of symbols, rituals, ceremonies, stories, and drama in political life (March, 1981; March & Olsen, 1976, 1983; Meyer & Rowan, 1977; Pfeffer, 1981a; Pondy, 1978). Symbols permeate politics in a subtle and diffuse way, providing interpretive coherence to political life. Many of the activities and experiences of politics are defined by their relation to myths and symbols that antedate them and that are widely shared. At the same time, symbolic behavior is also a strategic element in political competition. Individuals and groups are frequently hypocritical, reciting sacred myths without believing them and while violating their implications. The traditional problem with such observations is not doubt about their veracity but about our ability to translate them into useful theoretical statements without excessive damage to their meaning. Theoretical development reflective of an institutional perspective would include an examination of the ways in which the tendencies toward consistency and inconsistency in beliefs affect the organization of political meaning, the ways in which "exemplary centers" (Geertz, 1980) create social order through ceremony, and the ways in which symbolic behavior transforms more instrumental behavior and is transformed by it. In particular, a serious theoretical understanding of myths, symbols, and rituals must include some attention to the dynamics of symbols, to the processes by which symbols shape the behavior not only of the innocent but of the society as a whole.

Examples of Possible Theoretical Research

Within these six conceptions of order, there are possibilities for theoretical research attentive to the insights of students of institutions. Such research is institutional in two respects: First, it is oriented to one or more of the institutionalist conceptions of order; second, it tries to illuminate how institutional and organizational factors affect political events. As examples, consider the following:

Example 1: Policy Martingales. Many models of history recognize that specific historical events involve elements of chance. The unique historical happening may be a draw from some probability distribution of possible events. Even in cases where chance, strictly considered, is not viewed as vital, any specific event is seen as the consequence of a complicated interweaving of factors impossible to predict with precision in a single case. In the independent trial version of such models, any specific historical event is subject to various kinds of random fluctuations but, in the long run, unlikely events at a particular time are balanced by different unlikely events at a subsequent time. The specific realizations of the historical process that comprise the events of today are independent of the specific realizations that comprise the events of yesterday. Each specific event of an unfolding history is relatively difficult to predict, but prediction is not improved by knowledge of the history of past realizations of that process.

It is possible to see political policymaking as an independent trial process. Suppose we think of policy as the result of bargaining among political actors with prior preferences and resources, but subject to trial-by-trial variation attributable to specific, unpredictable and uncontrollable factors. Then understanding the short-run outcomes of a policy process would depend on considerable detail of the specific situation. A student of institutions might well observe that the details of the way attention is organized, how alternatives are presented, what information is available, which participants are free from other demands, how institutional memory is consulted, and a host of other factors would affect the specific political policy adopted at a specific time. At the same time, however, such factors are irrelevant (or redundant) to understanding the long-term mix of policies. Such an understanding is possible simply from a knowledge of the underlying political process and any systematic institutional biases.

Not all policymaking processes are independent trial processes. Many of them seem to be more in the nature of martingales (Feller, 1950). Like an independent trial process, a martingale process is subject to chance variation, but the variations accumulate. What distinguishes a martingale is the property that the *expected value* of the process at one time is equal to the *realization* of the process at the preceding time. This property makes the specific path of history important to understanding current historical events. In effect, the chance fluctuations of history change the baselines of the next step of the historical process. Common descriptions of incremental policy processes make them appear to be in the nature of martingales. The distribution of possible outcomes from a policy process is pictured as resulting from competition and bargaining over incremental adjustments in the current policy; the policy actually adopted is a draw from that distribution. This martingale property of policymaking is not independent of institutional factors. Indeed, it seems a prototypic institutional characteristic. Policies, once adopted, are embedded into institutions. They are associated with rules, expectations, and commitments. By affecting attention and aspirations, they affect the future search behavior of political participants.

Martingales diverge more rapidly than do independent trial processes; that is, for a given amount of chance variation in each time period, the variance across possible outcomes after some number of periods will be substantially greater in a martingale. As a result, the precision with which specific realizations of the process can be anticipated is considerably less. Thus, policy martingales are related to, but not identical to, various less precise ideas of forks in history, of critical events that made a difference. There is a sense in which the first step is more important than any subsequent one, but it is a limited sense. In a martingale process all events are forks; the policy paths of two political systems with identical underlying political conditions will be radically different simply because of the way in which (possibly small) perturbations shift the focus of political pressure.

Example 2: Experiential Learning. It is a frequent observation of institutionalism that institutions accumulate historical experience through learning. The results and inferences of past experience are stored in standard operating procedures, professional rules, and the elementary rules of thumb of a practical person. These elements of historical knowledge have been portrayed both as forms of irrational retrogression and as carriers of wisdom, and it is not hard to specify environmental situations in which either characterization would be appropriate. What is less clear is whether we can model the processes of institutional learning. Although there have been some loose arguments that experiential learning will, in the long run, lead to the discovery and adoption of optimal strategies, little theoretical effort has been devoted to specifying precisely the conditions under which learning from experience leads to optimal behavior, or to relating those conditions to features of institutional structure or life.

Consider the following simple model of learning (Levinthal & March, 1982). A decision-making institution simultaneously learns along three dimensions. First it modifies its strategy; that is, it changes the likelihood of making one choice rather than another among the alternative activities available to it. Subjective success leads to increasing the chance of repeating a choice; subjective failure leads to decreasing the chance of repeating a choice (March & Olsen, 1976). Second, an institution modifies its competences; that is, it changes the skill it has at the various activities in which it might engage. Competence at an activity increases with experience at it; it decreases with time (Preston & Keachie, 1964). Third, an institution modifies its aspirations; that is, it changes its definition of subjective success. Aspirations move in the direction of past performance (Cyert & March, 1963). It is clear that institutional factors affect several of the key features of such learning. The learning rates associated with the three kinds of learning are partly a function of features of the institution. The degree of loose coupling in an organization affects the precision with which choices are made, outcomes observed, aspirations expressed, and competences realized. Thus, it can be expressed as various forms of noise in the process. Organizational slack affects the degree of centralization in the organization, and thus the linkage among subunits.

The three dimensions of learning obviously interact. For example, learning of aspirations affects the definition of subjective success, and thereby affects the learning of strategies. Learning of competences affects performance outcomes, and thereby affects the learning of both strategies and aspirations. Learning of strategies affects choices, and thereby affects the learning of competences. The model can be explored to discover the circumstances under which it reaches an equilibrium, and, among those the circumstances, those under which it reaches an optimum. It can also be combined into more complicated structures of learning where the choices of one institution affect the outcomes of another (e.g., competition and cooperation), and where the learning institution is itself composed of learning subunits.

Example 3: Garbage Cans. Garbage-can models of organizational choice have been suggested as a representation of a particular temporal order. In the form most commonly discussed in the literature, the garbage-can model assumes that problems, solutions, decision makers, and choice opportunities are independent, exogenous streams flowing through a system (Cohen, March, & Olsen, 1972). They come together in a manner determined by their arrival times. Thus, solutions are linked to problems primarily by their simultaneity, relatively few problems are solved, and choices are made for the most part either before any problems are connected to them (oversight), or after the problems have abandoned one choice to associate themselves with another (flight). This situation of extreme loose coupling, called an open structure in the original discussions of the garbage can, has attracted most of the attention in the literature, and empirical studies have revealed decision processes that appear to approximate such an open structure (March & Olsen, 1976).

Not all decision situations are quite so unstructured, however. We can characterize a choice situation in terms of two structures. The first is the access structure, a relation between problems (or solutions) and choice opportunities. The access structure may require, allow, or not allow a particular problem, if activated, to be attached to a particular choice. The second structure is the decision structure, a relation between decision makers and choice opportunities. This structure may require, allow, or not allow that a particular decision maker participate in the making of a particular choice. Access and decision structures can be imagined in any kind of arbitrary configuration, but two special forms have been considered formally. A specialized structure is one that is decomposed into substructures that are open. Thus, a specialized decision structure is one in which it is possible to divide choice opportunities and decision makers into subgroups and match the two sets of subgroups so that every decision maker in a particular subgroup of decision makers has access to every choice opportunity in the matched set of choice opportunities, but to no other. A hierarchical structure is one that expands access rights as a function of hierarchical rank. For example, in a hierarchical access structure, problems and choices are ordered, and each problem has access to choices of the same or lower rank. The differences made by these structures have been noted both formally (Cohen, March, & Olsen, 1972) and empirically (Egeberg, 1981; Olsen, 1983), but the empirical and theoretical examination of garbage-can processes within access and decision structures that are not completely open is barely begun.

Subtle Phenomena and Simple Theories

These examples hardly exhaust the list. Empirical observations of reputations for power in politics suggest that such reputations depend heavily on the place of an individual in a political structure and on inferences about the relation between preferences and outcomes. Some simple models of the dynamic relations among reputations for power, institutional position, preferences, and social outcomes would provide a richer understanding of the ways in which power reputations affect politics. Empirical observations of post-decision surprises (i.e., deviations of realized outcomes from expected outcomes) suggest that there are systematic differences between the ways in which individuals experience the consequences of their actions and the ways in which institutions do. Some simple models of institutional expectations, choices, and post-decision assessments would clarify the occasions for expecting positive or negative surprises from deliberate action.

What characterizes all of the examples, as well as the others that might be added, is a relatively simple approach to institutional phenomena. The new institutionalism is often couched in terms of a contrast between the complexity of reality and the simplifications provided by existing theories, but theoretical research from an institutional perspective cannot involve the pursuit of enormous contextual detail. It is constrained by the capacity of human (and artificial) intelligence to cope with complexity, and although that capacity seems to expand with time, the rate of expansion continues to be modest relative to the demands of a fully contextual and institutional theory. From the point of view of theoretical research, consequently, the new institutionalism is probably better viewed as a search for alternative ideas that simplify the subtleties of empirical wisdom in a theoretically useful way.

Conclusion

The institutionalism we have considered is neither a theory nor a coherent critique of one. It is simply an argument that the organization of political life makes a difference. Some of the things we have noted are fragments of ideas; others are somewhat more systematic in developing a theme or reporting a series of observations. They are held together by an awareness of a set of phenomena that are more easily observed than explicated. Insofar as the ideas are consistent, the consistency is sustained partly by ambiguity. Many of the core ideas seem plausible and have been durable, but plausible durability (as numerous students of the history of knowledge have observed) is neither necessary nor sufficient for good sense.

The new institutionalism is an empirically based prejudice, an assertion that what we observe in the world is inconsistent with the ways in which contemporary theories ask us to talk. Like other prejudices in knowledge, it may be wrong-headed or muddle-headed, but it may also be a useful continuation of that gentle confrontation between the wise and the smart that describes much of intellectual history. On the chance that it is the latter, which of course does not exclude the possibility that it is also the former, we have tried to draw some possible implications for theoretical research in political science. They are, at best, theoretical directions suggested by a sympathetic appreciation of a tradition of institutionalist thought. Such an effort is a little like trying to write a useful commentary on Heidegger in the form of a Shakespearean sonnet. If it has virtue, it is in attempting to encourage talking about a subtle body of thought in a way sufficiently naive to entice the technically proficient.

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Ordinal Preference Games: An Analysis of Legislative Coalition Formation Without Transferable Utility

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Two important findings from n*-person game theory are that coalition formation is superadditive if and only if a game possesses an empty core, and Riker's size principle. Up to the present, both theorems have been proved and critiqued under the assumption of transferable cardinal utility.*

This analysis eliminates the cardinal utility assumption on the grounds that it is generally inconsistent with the conditions that characterize collective choice on political issues. Instead, a model of collective decision making is set forth in terms of ordinal preferences, and the two theorems are reconsidered. The superadditivity theorem survives intact, whereas the size principle is weakened. Circumstances are identified in which there is no incentive to reduce an oversized winning coalition; however, under no condition is there an incentive to increase the size of a winning coalition. A number of coalition-size hypotheses are tested using roll-call data from the U.S. Senate. The results raise new questions about the role of coalition formation in legislative voting strategy.

Few theoretical statements about political behavior have received more attention than William Riker's (1962) size principle. The idea that coalition formation is a fundamental element in political choice processes is intuitively appealing; that it is possible under certain conditions to deduce a hypothesis about size, an important parameter of all coalitions, is all the more impressive.

Since the publication of *The Theory of Political Coalitions,* the size principle has been the subject of theoretical controversy (Hardin, 1976, reviews the debate up to that time), and has achieved mixed success on the empirical front (Hardin, 1976; Thompson, 1979). The theoretical argument proceeds from Riker's initial formulation, where it is assumed that utility is transferable and that side payments are permitted. Under those conditions Hardin shows that the size principle can be proved only for two restrictive classes of games: supersymmetric games and asymmetric games where the sum of payoffs to the winning coalition is equal to the negative sum of the security levels of the losers (1976, p. 1207).

Considering applications based on legislative roll-call data, Hardin goes on to say that the size principle is all but untestable (1976, p. 1213). A fair test requires the statement of a plausible null hypothesis, a prediction of the outcome of the test should it fail. Because a random voting model implies a sampling distribution such that minimal majorities are among the most probable outcomes, the obvious null hypothesis is indistinguishable from the minimum winning coalition hypothesis. In response, Thompson (1979, p. 1106) argues that a fair test could be achieved by observing legislative voting outcomes under different decision rules. If coalition size is responsive to different majority requirements, specifically including cases where minimal winning size is greater than 50% + 1 of those present and voting, it may be possible to reject the random voting hypothesis.

In general, applications of the size principle fail to confront two serious problems. First, the theory includes assumptions that are not consistent with political decision making and, specifically, legislative voting decisions. Second, empirical tests are flawed by imprecise definitions of what actually constitutes coalition formation and by failure to utilize a plausible null hypothesis.

In this article, an alternative approach to political coalition formation is proposed and tested using legislative roll-call outcomes. The point of departure is that the assumption of transferable cardinal utility is not appropriate to the analysis of collective political decisions. Thus, in the first section a model of coalition formation is developed in which it is assumed that individual preferences are limited to ordinal rankings of elements in an outcome set and that there are no side payments. Then two theorems are proved; a restatement of the Von Neumann-Morgenstern theorem

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(1944, p. 280), which relates the existence of a core (an undominated majority outcome) to inessential (nonsuperadditive) games, and a restatement of the size principle.

In the second section these results are applied to legislative voting decisions. The Von Neumann-Morgenstern theorem proves that not all roll-call decisions are susceptible to rational, superadditive, coalition formation. Thus, a distinction is made between the conventional concept of implicit coalition formation—that the simple coincidence of votes among a set of legislators is a sufficient reason to conclude that a coalition has been formed—and explicit coalition formation—the conscious adoption by a set of legislators of joint voting strategies. A number of structural conditions are established from the theorem, under which there is no incentive to form explicit legislative coalitions. Because the size principle applies only to explicit coalition formation, it is possible to identify roll calls where there is no probability of coalition size effects, and others where minimal size is increasingly likely. This leads to the statement of a number of hypotheses regarding the mean size of roll-call outcomes for different categories of legislative voting decisions. These are based on a more plausible null hypothesis than Hardin's random voting assumption and do not depend on the infrequent and unpredictable use of special majority decision rules.

The initial test of the model, using 1981 Senate roll-call data, yields some interesting results, if less-than-compelling confirmation of the coalition-size hypothesis. The article concludes with a discussion of the results and consideration of the possibility that the difficulty in observing size effects may result from extremely limited opportunities for rational legislative coalition formation.

N-Person Game Theory and Political Coalitions

Riker's size principle is deduced from a set of game-theoretic assumptions:

> In *n*-person zero-sum games, where side payments are permitted, where players are rational and where they have perfect information, only minimum winning coalitions occur. (1962, p. 32)

On first consideration this statement seems intuitively obvious. Coalition members in excess of the number necessary to win are superfluous at best, and costly if they are able to claim a share of the payoff. However, a review of the debate among Butterworth (1971), Hardin (1976), Riker (1971), and Shepsle (1974) demonstrates that the question of coalition size is anything but obvious.[1] The range of circumstances in which the size principle does not hold is indicated by Hardin, who shows that

> In general there will always be an incentive for a winning coalition to expand *except* when the value of that coalition is equal to the negative sum of the security levels of the individual losers. (1976, p. 1206)

This restrictive conclusion can be traced to the game-theoretic assumptions from which the size principle is derived. Specifically, the zero-sum condition entails the implicit assumption of transferable (side payments) cardinal utility. McKelvey, Ordeshook, and Winer note that this assumption is part of conventional *n*-person game theory, including the derivation of the size principle, and also "that the current debate over the size principle's theoretical validity assumes transferable utility" (1978, p. 599). The current debate notwithstanding, the outcome of political policy decisions generally cannot be interpreted in terms of quantitative utility; it is not possible to "distribute the outcome" among some individuals and to exclude others.

A second theorem, attributable to Von Neumann and Morgenstern (1944, p. 280), is directly related to applications of the size principle. Von Neumann and Morgenstern proved that if the core of an *n*-person, zero-sum game is nonempty, the game is inessential, and the converse.[2] A nonempty core means that among the outcomes of a game there is one preferred by a majority of the players to all other outcomes. A game that is inessential is not superadditive; there is no payoff strictly attributable to coalition formation. Thus, the theorem proves that if there is an outcome preferred by a majority to all others, no payoff can be achieved by forming a coalition which cannot be achieved without forming a coalition. Riker contends that this eliminates the incentive to join a coalition:

> Suppose, however, it were always true that $v(S \cup T) = v(S) + v(T)$ . . . it would be difficult to imagine the occurrence of coalition formation

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[1] The debate ranges over a variety of issues, but none more salient than the distribution of the payoff among members of a winning coalition. In this article it is assumed that political choices generally concern nondivisible public goods: The outcome is the same for each individual. This eliminates the distribution question.

[2] The authors' reference to a "one-element solution" is equivalent to what is now commonly called a majority outcome, a Condorcet winner, or an outcome in the core.

> inasmuch as rational players would not expend energy to change position if the most they could expect would be the same payoff. . . . It is clear that for a game in which $v(S \cup T) = v(S) + v(T)$ there is no point to forming coalitions. (1962, p. 250)

If from time to time legislative decisions include a nonempty core, the Von Neumann-Morgenstern theorem would appear to have profound implications for tests of the size principle using roll-call data. It tells us that there is an incentive to form coalitions on some, but not all, roll calls. If there is no rational purpose to forming a coalition on a legislative decision, the question of coalition size is moot. Yet this theorem, like the size principle, is proved using the assumption of transferable cardinal utility. Consequently, its application to political decision making is equally suspect.

A number of variations on conventional *n*-person game theory have been proposed which yield more plausible models of legislative voting decisions: simple games (Owen, 1968, p. 163), games without side payments (Owen, 1968, p. 199), the Competitive Solution (McKelvey et al., 1978), the minimax set (Simpson, 1969), and the least vulnerable set (Ferejohn et al., 1978). Still, none of these directly confronts the question of conditions for legislative coalition formation and optimal coalition size. In what follows, several assumptions from *n*-person game theory are reformulated in terms of ordinal preferences, and then are used to establish the validity of the Von Neumann-Morgenstern theorem and the size principle for application to sequential voting decisions.

Ordinal Preference Games

Assume there is a set of players, $N = \{1,2, \ldots, n\}$, and a set of potential outcomes that form the agenda of a collective choice, $A = \{x,y, \ldots\}$. The agenda is a discrete subset of X, the set of feasible outcomes, whereas X is a subset of all conceivable outcomes in an m-dimensional issue space, R^m. Hence, $A \subseteq X \subseteq R^m$.[3]

Each $i \in N$ is assumed to have a strong preference order over A. Individual preference relations are denoted by P_i, which is assumed to be irreflexive, connected, and transitive ($x\bar{P}_iy$ means x is not preferred to y by player i). To make a collective choice, players express their preferences over elements in the outcome set by voting. They may either vote as individuals, or they may coordinate their voting strategies, in which case they will be said to have formed a coalition, $S \subseteq N$. The size of a coalition is indicated by $|S|$.

To emphasize that this analysis includes no assumptions about transferable utility, reference is made to ordinal preference games. An ordinal preference game is a pair (v,A), where v is a set-valued function that assigns a subset of A to each $S \subseteq N$. The set of outcomes, $v(S) \subseteq A$, specifies the worst the members of a coalition may expect to do against the best efforts of the remainder of their colleagues.[4] This version of $v(S)$ is not the same as a characteristic function, which is a real valued function indicating "the amount of utility that the members of S can obtain from the game" (Owen, 1968, p. 157). Once the assumption of transferable utility is dropped, the concept of the "worst" a coalition can do is likely to be ambiguous. If members of a coalition have different preference orders over the outcome set, it may not be possible to specify a single worst outcome. Thus, $v(S)$ as used in this analysis is comparable to the McKelvey et al. payoff function, which is defined as a subset of the outcome space as opposed to a unique point in the utility space (1978, p. 602).

The formal definition of $v(S)$ depends on whether or not $S \in W$, the set of winning coalitions:

$$W = \{S \subseteq N \mid |S| > N/2\}.$$

Then for each $S \in W$,

$$v(S) = \{x \in A \mid \nexists\, y \in A,\ y \neq x \text{ and } yP_ix\ \forall\ i \in S\},$$

and for $S \notin W$, $v(S) = A$. These definitions are consistent with the principle of majority rule that renders minorities powerless and majorities all-powerful. The set of outcomes $v(S)$, $S \notin W$ includes the entire agenda because, regardless of their preferences, members of a minority are unable to prevent imposition of any outcome by their colleagues. Alternatively, winning majorities are all-powerful in the sense that they can legitimately designate any outcome in A to be the collective choice; however, their actions are constrained by the preferences of their members. The set of outcomes $v(S)$, $S \in W$, is a subset of A which excludes those outcomes $y \in A$ that are considered inferior to some $x \in A$ by all $i \in S$. Thus, the worst that can happen to the coalition that wins does not include any outcome in A which is unanimously dominated by some other outcome according to the preferences of the members of that coalition.

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[3] The conventional notation $A \subseteq X \subseteq R^m$ does not completely capture the concept of an agenda as the term is used in this article. First, the notation admits the possibility that A could contain every point in the issue space R^m. It is important to include this case, since it does not affect the results which follow, and thus emphasizes the generality of the theorems. Second, the notation also admits the possibility that A could contain as few as two of the outcomes (a single-issue decision) that compose R^m. It is the "few-outcome" agenda that most effectively models legislative decisions in the real world, and which generates the results that set this article apart from its predecessors in the social choice literature.

[4] "Worst" as used in this context is analogous to the "security level" in transferable utility games. The members of $S \subseteq N$ need not accept any outcome that they agree is inferior to any outcome in the set $v(S)$. This formulation of $v(S)$ was suggested by a unique characteristic function representation that appears in Riker and Ordeshook (1973, pp. 137-138).

Several more definitions are necessary to translate concepts typically expressed in terms of cardinal utility into ordinal preference language. A limited payoff game is one in which there is no outcome $y \in A$ such that $\forall\, i \in N\colon y\, P_i x$, $\forall\, x \in A - y$. This is analogous to the constant sum condition. It excludes decisions where some outcome is preferred by all legislators to all other outcomes, thereby assuring some conflict over the collective choice.

An essential n-person, k-sum game is superadditive; the value of at least one coalition exceeds the sum of the values of its subsets. In the absence of transferable utility, essential games are defined in terms of the capability of a coalition to reduce the size of the sets of potential worst outcomes that confront its members. That is, an ordinal preference game is essential if $\forall\, x \in A$ there is some $R, S \subseteq N$, $R \cap S = \phi$, such that $x \in v(R)$, $x \in v(S)$ but $x \notin v(R \cup S)$. This means that for every $x \in A$, there is some coalition $S \in W$ such that $x \notin v(S)$. It follows that if a game is essential $\exists\, R, S, \subseteq N$ such that $v(R \cup S)\ P_i\ v(R)\ \forall\, i \in R$, and that $v(R \cup S)\ P_i\ v(S)\ \forall\, i \in S$. Alternatively, an ordinal preference game is inessential if $\exists\, x \in A$ such that $\forall\, R, S \subseteq N$, $R \cap S = \phi$, $x \in v(R)$, $x \in v(S)$ and $x \in v(R \cup S)$. It follows that if a game is inessential, for some $x \in A$ and $\forall\, R, S \subseteq N$, $v(R \cup S)\, \bar{P}_i v(R)\ \forall\, i \in R$, and $v(R \cup S)\, \bar{P}_i\, v(S)\ \forall\, i \in S$.[5]

Finally the core of an ordinal preference game is an undominated outcome. Thus, we define

$$C = \{x \in A \mid \forall\, S \in W \text{ and } y \in v(S),$$
$$y \neq x,\ \exists\, i \in S \text{ such that } xP_iy\},$$

which says that there is no $y \in A$ preferred by all members of a winning coalition to $x \in C$.

Two Results for Ordinal Preference Games

With the terminology identified, consider the two theorems from n-person game theory noted above. The original theorem expressing the relation between the existence of a core and superadditivity was stated by Von Neumann and Morgenstern as follows:

> A [n-person, zero-sum] game possesses a one-element solution if and only if it is inessential. (p. 280)

A one-element solution refers to an outcome preferred by a majority to all other outcomes: that is, the core. Restating the theorem in terms of ordinal preferences,

> *Theorem 1:* For n-person, limited payoff ordinal preference games, there is an outcome in the core if, and only if, the game is inessential.
>
> *Proof:* Assume $x \in C$. Thus, $\forall\, S \in W$ and $\forall\, y \in v(S)$, $\exists\, i \in S$ such that $x\, P_i y$. From the definition of $v(S)$ it follows that $\forall\, S \subseteq N$, $x \in v(S)$. Now consider all $R, S \subseteq N$, $R \cap S = \phi$. Since $x \in v(R)$, $x \in v(S)$, and $x \in v(R \cup S)$, there is an outcome such that $v(R \cup S)\, \bar{P}_i v(R)\ \forall\, i \in R$, and $v(R \cup S)\, \bar{P}_i v(S)\ \forall\, i \in S$. Such a game is inessential.
>
> Conversely, assume that $C = \phi$. It follows from the definition of the core that $\forall\, x \in A$ there is some $S \in W$ and $y \in v(S)$ such that $\forall\, i \in S$, yP_ix. Consequently, $\forall\, x \in A$, there is some $R, S \subseteq N$, $R \cap S = \phi$, such that $x \in v(R)$, $x \in v(S)$, and $x \notin v(R \cup S)$. Thus, $v(R \cup S)\, P_i v(R)\ \forall\, i \in R$, and $v(R \cup S)\, P_i v(S)\ \forall\, i \in S$: the game is essential.

The proof of theorem 1 shows that the relationship between the existence of a core and inessential games does not depend on transferable cardinal utility. Thus, it is applicable to political decision making analyzed in terms of ordinal preferences. The next question concerns the size principle: Given the same assumptions, is there an incentive to limit the size of winning coalitions? Riker's original statement of the principle is very strong, suggesting that the drive to minimize coalition membershp is all but universal. Using the definitions developed above, it is at least possible to prove a weak statement of the size principle:

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[5] An essential game in the conventional definition is one in which the formation of some coalition enables the subsets of that coalition to do better than their security level. The same is true of an essential ordinal preference game where for all possible outcomes, x, there is some $S \in W$ such that $x \notin v(S)$. Thus, the subsets of S do better than their security level A. If an ordinal preference game is inessential, there is some outcome x such that $x \in v(S)$ for all $S \subseteq N$. Thus, no coalition, winning or losing, can do better than x because it is among the set of worst outcomes, $v(S)$, for every subset of N.

> *Theorem 2:* For n-person limited payoff ordinal preference games, members may gain from decreasing, but not from increasing, the size of a winning coalition.

> *Proof:* Consider a coalition $S \in W$ and $v(S) = \{x, y, \ldots\}$. Assume that $j \in S$ departs from S, and that $S - \{j\} \in W$. If $x \in v(S)$ and $y \in v(S)$ and $\forall\ i \in S - \{j\}$, xP_iy, then $y \notin v(S - \{j\})$. Thus $v(S - \{j\})\ P_i\ v(S)\ \forall\ i \in S - \{j\}$: all members of $S - \{j\}$ gain as a result of j's departure from S.
>
> To show that an increase in the size of $S \in W$ never results in a gain for all $i \in S$, consider an individual player, $j \in N - S$. Assume that the members of $S \in W$ can gain by adding $\{j\}$ to their coalition: $\forall\ i \in S$, $v(S + \{j\})\ P_i\ v(S)$. It follows that $\exists\ y \in v(S)$ such that $y \notin v(S + \{j\})$. Thus, for some $x \in v(S)$, $\nexists\ i \in S + \{j\}$ such that $y\ P_ix$. But from the definition of $v(S)$, $S \in W$, if $y \in v(S)$ there is no $x \in v(S)$, $x \neq y$, such that $xP_iy\ \forall\ i \in S$. Thus, the assumption $v(S + \{j\})\ P_i\ v(S)\ \forall\ i \in S$ leads to a contradiction. It follows that members can never gain by increasing the size of an already winning coalition.

Discussion

Theorems 1 and 2 prove that even without transferable utility, the relation between the core and essential games survives, as does a weak version of the size principle. Two examples, sequential voting decisions with five voters and a three-issue binary voting procedure, help to clarify the definitions and results. In Figures 1 and 2 outcomes are represented using notation that indicates acceptance (1) or rejection (0) of elements of the set of issues that form a voting decision. For example, $100 \in A$ means that the voters have rejected two issues and approved the third. For Figures 1 and 2 with three issues, $A = \{000\ 001\ 010 \ldots 111\}$.

In Figure 1 the outcomes in parentheses on the diagram show the majority winner at each potential division.[6] Thus, 000 is preferred by a majority to 100 and 110, but not to 001. The latter is the survivor of majority voting over the three issues. It is referred to as the anticipated outcome—the outcome expected by the voters if no coalition is formed. The anticipated outcome of a sequential voting decision is a critical factor in rational coalition formation. It partitions the set of winning coalitions into two subsets: those that, if formed, provide unambiguous gains for their members, and those that do not. That is, if x is the anticipated outcome, $\forall\ S \in W$ such that $x \notin v(S)$, coalition formation is beneficial for all members. However, $\forall\ S \in W$ such that $x \in v(S)$, there is no outcome which is preferred by all members of S to the anticipated outcome.

Below the diagram in Figure 1, $v(S)$ is listed for each $S \subseteq N$. Note that $v(S)$ for $|S| = 1$, and 2 includes all outcomes in A because minorities are powerless to prevent imposition of any outcome. Alternatively, $v(345)$ is a proper subset of A because the members unanimously prefer 111 to 001, 010, and 100. In effect, $S = \{345\}$ has the incentive and the power to block all of the potential outcomes that do not appear in $v(345)$. Those that remain form the set of potential worst outcomes in the sense that $S = \{345\}$ cannot agree to block their imposition. That is because no outcome $x \in v(345)$ is preferred by all $i \in \{345\}$ to any other $y \in v(345)$.

In general, the presence of more than a single outcome in $v(S)$ indicates disagreement among coalition member preferences. If all members of some $S \in W$ have identical preference orders, $v(S)$ contains a single element: the top choice for all $i \in S$. Alternatively, there is no reason that all members of a coalition $S \in W$ must rank any $x \in A$ above any $y \in A$. In the absence of preference agreement, $v(S) = A$. The extent of disagreement among the members of S determines what coalition formation can and cannot achieve. Formation of $S = \{125\}$ cannot prevent the imposition of $101 \in v(125)$ even though it is the last choice of both I_1 and I_2. What $S = \{125\}$ can do is respond to the threat of an outcome such as $010 \in v(12)$, $010 \in v(5)$ with the unanimously preferred $011 \in v(125)$.

Considering Theorem 1, note that the example in Figure 1 has an empty core: No outcome is preferred by a majority to all other outcomes. It follows that the voting decision forms an essential game; this can be confirmed by noting that every potential outcome is excluded from $v(S)$ for some $S \in W$. Thus, no matter what $x \in A$ is the anticipated outcome, there is some coalition $S \in W$ such that $x \notin v(S)$.

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[6] The method of majority voting used here assumes that voters choose optimally among elements in the outcome set. This is achieved by considering the voters' sincere preference at each possible final vote (in Figure 1, 000 vs. 100, 010 vs. 110, etc.), and using those sincere majority outcomes to form the choice set for each possible second-to-last vote. This is continued until the sincere choice on the first vote is determined, which is the outcome of the voting decision. This method, which yields a Nash equilibrium, is developed in McKelvey and Niemi (1978). It is one version of sophisticated voting, and it is assumed for all voting applications considered in this article.

If there is an incentive to form coalitions, the question of size is germane. The example in Figure 1 illustrates the weak size effect proved by Theorem 2. To determine whether a winning coalition of one size is preferred to a winning coalition of another size, it is helpful to note the number of outcomes listed in $v(S)$. For example, since $v(1235) = \{000\ 001\ 011\ 101\ 110\ 111\}$ and $v(123) = \{001\ 110\ 111\}$, it follows that $v(123)\ P_i\ v(1235)$ for all $i \in \{123\}$.[7] Thus, the members $i \in \{123\}$ have an incentive to exclude $\{5\}$ from $\{1235\}$. Also note that $v(125)\ P_i\ v(1235)$ $\forall\ i \in \{125\}$; however, it is not universally bene-

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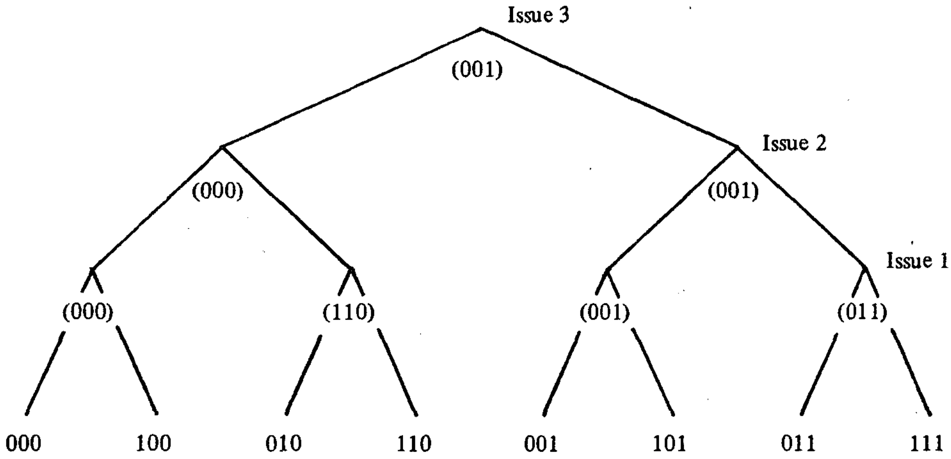
[7] It is assumed that in general members of a would-be winning coalition prefer smaller to larger sets of worst outcomes, $v(S)$. In practice, a coalition is of value to its members only if the anticipated outcome $x \notin v(S)$, and S actually forms.

Figure 1.



Voter Preference Orders

I_1	I_2	I_3	I_4	I_5
001	001	111	111	101
011	011	110	110	000
000	000	001	001	011
110	110	000	000	110
111	111	011	011	111
100	100	100	100	001
010	010	101	101	010
101	101	010	010	100

$v(S)$

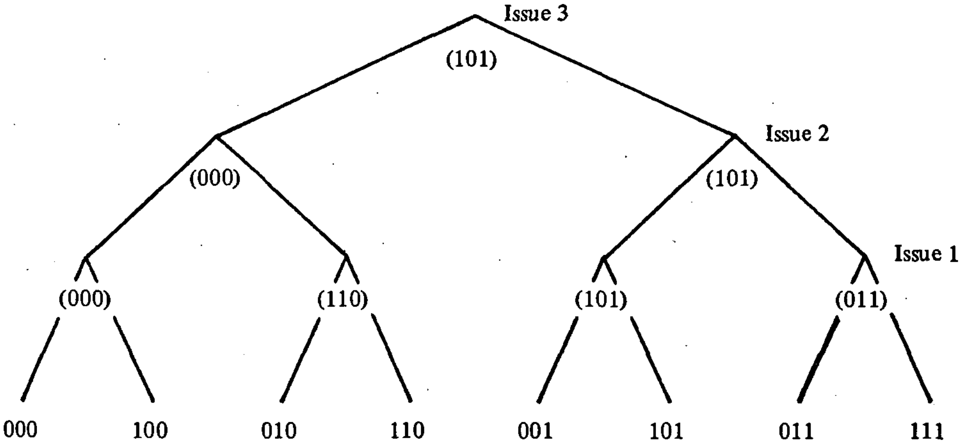
$v(123) = \{$— 001 — — — — 110 111$\}$
$v(124) = \{$— 001 — — — — 110 111$\}$
$v(125) = \{$000 001 — 011 — 101 — —$\}$
$v(134) = \{$— 001 — — — — 110 111$\}$
$v(135) = \{$000 001 — 011 — 101 110 111$\}$
$v(145) = \{$000 001 — 011 — 101 110 111$\}$
$v(234) = \{$— 001 — — — — 110 111$\}$
$v(235) = \{$000 001 — 011 — 101 110 111$\}$
$v(245) = \{$000 001 — 011 — 101 110 111$\}$
$v(345) = \{$000 — — 011 — 101 110 111$\}$

$v(i) = v(ij) = \{A\}\quad i,j = 1, \ldots, 5\quad i \neq j$

$v(1234) = \{$— 001 — — — — 110 111$\}$
$v(1235) = \{$000 001 — 011 — 101 110 111$\}$
$v(1245) = \{$000 001 — 011 — 101 110 111$\}$
$v(1345) = \{$000 001 — 011 — 101 110 111$\}$
$v(2345) = \{$000 001 — 011 — 101 110 111$\}$

$v(12345) = \{$000 001 — 011 — 101 110 111$\}$

Figure 2.



Voter Preference Orders

I_1	I_2	I_3	I_4	I_5
001	000	101	001	111
011	011	111	010	101
000	101	100	101	010
110	100	110	111	100
111	110	001	000	001
100	001	011	011	110
010	010	000	110	011
101	111	010	100	000

$v(S)$

$v(123)$ = {000 001 --- 011 100 101 110 111}
$v(124)$ = {000 001 --- 011 --- 101 --- ---}
$v(125)$ = {000 001 010 011 100 101 110 111}
$v(134)$ = {--- 001 --- --- --- 101 110 111}
$v(135)$ = {--- 001 --- --- --- 101 110 111}
$v(145)$ = {--- 001 010 --- --- 101 --- 111}
$v(234)$ = {000 001 --- 011 000 101 --- ---}
$v(235)$ = {000 --- --- 011 --- 101 --- 111}
$v(245)$ = {000 001 010 011 --- 101 --- 111}
$v(345)$ = {--- 001 010 --- --- 101 --- 111}

$v(i) = v(ij) = \{A\} \quad i,j = 1, \ldots, 5 \quad i \neq j$

$v(1234)$ = {000 001 --- 011 100 101 110 111}
$v(1235)$ = {000 001 010 011 100 101 110 111}
$v(1245)$ = {000 001 010 011 100 101 110 111}
$v(1345)$ = {--- 001 010 011 100 101 110 111}
$v(2345)$ = {000 001 010 011 --- 101 --- 111}

$v(12345)$ = {000 001 010 011 100 101 110 111}

ficial to reduce coalition size. First, there is no incentive to eliminate either {1} or {2} from {1235} because $v(1235) = v(235) = v(135)$. Thus, the fact that one member can be excluded from an oversized coalition to the benefit of the remaining members does not imply that is true for all other members of the same coalition. Further, examination of $v(1234)$ shows that there is nothing to be gained by excluding any member of the oversized coalition: $v(1234) = v(123) = v(124) = v(134) = v(234)$.

On the other hand, inspection of $v(S)$, $\forall S \epsilon W$, shows that there is nothing to gain by increasing the membership of any winning coalition. This is consistent with Hardin's statement quoted previously on the incentive for winning coalitions to expand. Clearly the set of worst outcomes is only roughly analogous to the security level in a transferable utility game. Nevertheless, because $v(S) = A$, $\forall S \notin W$, no matter what coalition or outcome prevails, the losers are necessarily held to a worst outcome.

Figure 2 represents a similar three-issue decision, except that there is an outcome in the core: $101 \in A$ is preferred by a majority to all other outcomes. It follows from Theorem 1 that the decision is an inessential game, which is confirmed by noting that $101 \in v(S)$ for all $S \subseteq N$. If 101 is the anticipated outcome, there is no coalition $S \in W$ such that $101 \notin v(S)$, and thus no incentive to form a winning coalition of minimal or any other size.

This much is straightforward; however, the concept of inessential ordinal preference games is complicated by the fact that $v(S)$, $S \in W$, may include outcomes in addition to the core. Consequently, Figure 2 may appear to contain a counterexample to Theorem 1. For example, assume that 010 is the anticipated outcome. Then, since $010 \in v(12)$, $010 \in v(3)$ and because $010 \notin v(123)$, it follows that $v(123)\ P_i\ v(12)$, $i = 1{,}2$ and $v(123)\ P_i\ v(3)$, $i = 3$. This is consistent with the definition of an essential game even though there is an outcome in the core.

Nevertheless, this is neither a counterexample to Theorem 1 nor is it inconsistent with the original Von Neumann-Morgenstern theorem, because both theorems are proved with respect to the outcome in the core. In the transferable utility case, one assumes that an imputation consistent with the definition of the core exists, and then proves that the further assumption of superadditivity yields a contradiction (Luce & Raiffa, 1957, pp. 194-195; Owen, 1968, pp. 164-165; Von Neumann & Morgenstern, 1944, p. 280). For ordinal preference games, the implication is that if there is an outcome in the core, it is the anticipated outcome; the voters' expectation if no coalition is formed. This is a certainty if it is assumed that voters use optimal individual voting strategies.[8] Thus, if a voting decision includes an outcome preferred by a majority to all others, that outcome will be the basis for individual decisions on whether or not to form a coalition. From that benchmark Theorem 1 establishes that there is nothing to be gained from coalition formation.

In this section it has been shown how $v(S)$ is applied to sequential voting decisions. If the structure of voting is left unspecified, $v(S)$ indicates only the potential of a coalition to preclude the collective choice of outcomes $x \notin v(S)$. However, once the structure of a voting decision is specified (e.g., the binary choice procedure and the order of voting on issues), the anticipated outcome can be determined, and thus the coalitions $S \in W$ that have a chance to realize their potential. In the next section these principles are applied to legislative voting decisions.

Application to Legislative Voting Decisions

Legislative roll calls have considerable appeal to those interested in testing political theories, including the size principle. Legislative voting is a highly formalized and consistent process for making political decisions. It generates an abundance of accessible data and does so regularly over extended periods. These reasons, and the fact that on occasion legislators obviously do form coalitions, have led to the use of roll-call data without due consideration for the frequency of coalition formation. There appears to be an unstated assumption that if evidence to support the size principle exists anywhere, it certainly should be found in roll-call outcomes.

The results from the first section show that there are likely to be subsets of roll calls where this assumption does not hold. Clearly, the simple coincidence of votes among a group of legislators is not a sufficient reason to conclude that they have formed a coalition. The theory of games does not support this concept of implicit coalition formation, and neither does Riker's original formulation. In an appendix Riker discusses the game-theoretic basis of the size principle. At one point, referring to the set of players forming the complement of a winning coalition, he states: "The remaining players whether actually joined in an alliance or not, may conventionally be regarded as members of a single losing coalition" (1962, p. 248).

Here "conventionally" means that players who act together but do not explicitly coordinate their actions (are not "actually joined in an alliance") do not by definition constitute a coalition but for some purposes may be treated as such. Theorem 1 shows that this convention is positively unacceptable if roll-call data are to be used to evaluate coalition size effects.

The importance of Theorem 1 to the use of roll-call data for coalition analysis depends on how often legislative voting decisions include a majority outcome. The social choice literature informs us that highly restrictive conditions must be met in order to ensure the existence of a nonempty core (Plott, 1967); that is, given ordinary voter preference configurations, it is generally impossible to find any outcome that is not dominated by some other outcome in an issue space. From this one might conclude that an outcome in the core is a rarity, and that incentives to form coalitions are prevalent in legislative decision making. However, legislative agendas do not include all points in an issue space. Thus, the question is not whether an outcome on the agenda dominates all other possible outcomes, but rather, all other outcomes on the agenda. In legislative voting, decisions are focused on one bill at a time, and typically, one or two amendments to the bill. With such a limited agenda it may well be that a single outcome (in a set of 2, 3, or 4 possible outcomes) is preferred by a majority to all others.

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[8] McKelvey and Niemi (1978) proved that whenever the core is non-empty, selection of optimal individual voting strategies (see note 6) results in the collective choice of the outcome in the core.

Consideration of the structure of legislative voting decisions enables one to specify a number of conditions that either assure or affect the probability of existence of an outcome in the core. Legislative decisions entail a sequence of binary choices over a set of issues that form an agenda, e.g., a bill and one or more amendments. Issues are decided in the reverse order of their introduction, with the final choice between a set of issues (a bill with the approved amendments) and the status quo. Since amendments cannot finally be approved without the bill that they are proposed to amend, the analysis of legislative decisions requires a modification of the procedure shown in Figures 1 and 2. Outcomes 001, 010, and 011 are not admissible and are replaced in each case by 000. This modified binary voting procedure is the basis of a number of propositions regarding the existence of an outcome in the core.

There are two simple conditions that assure the existence of an undominated outcome. First, if a legislative voting decision is limited to a single issue (a motion or bill with no amendments), one of the two possible outcomes (the issue and the status quo) must be preferred by a majority to the other. Because that outcome is not majority dominated by any other outcome on the agenda, it satisfies the definition of the core. Second, assume that issue by issue legislators make optimal use of their individual votes. In that case, if the collective choice is the status quo, then it is in the core. This follows from the modified binary voting procedure in which a bill or resolution, however amended, must finally gain approval of a majority against the status quo. Thus, if the status quo survives, it must be preferred by a majority to all other outcomes.

Two additional conditions regarding the spatial location of issues affect the probability of existence of an outcome in the core. Figure 3 is a representation of a convex hull of ideal points (CHIP) for five legislators in a two-dimensional issue space, with 000, 100, $110 \in A$ respectively indicating the location of the status quo, a bill, and the bill with one amendment.

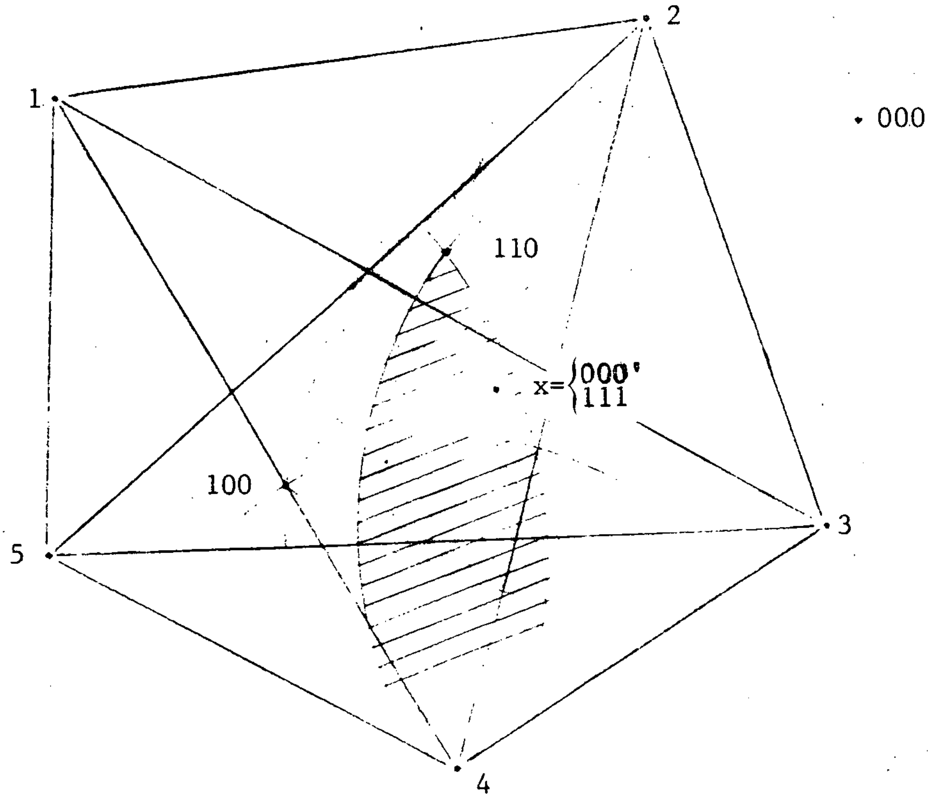
As noted previously, a one-issue decision always includes an outcome in the core. Thus, introduction of an amendment to a bill, thereby forming a two-issue decision (with three possible outcomes), is a necessary condition for the existence of an empty core. It is not, however, a sufficient condition. The shaded areas in Figure 3 locate sets of amendments that are preferred by a majority to the bill (100). Note that because of the distance of the status quo (000) from the CHIP, the bill as amended (110) is preferred by a majority to the status quo as well as the bill. Thus $110 \in C$, and the legislative decision forms an inessential ordinal preference game.

Either one of two changes is sufficient to eliminate the core and thereby create an incentive for rational coalition formation. First, if the status quo is not so remote, it may be preferred by a majority to the bill as amended (110). The cross-hatched area identifies a subset of the points in the issue space that are preferred by a majority to the bill as amended. Assume $x = 000'$ represents an alternative location for the status quo, whereas the bill and the amended bill remain unchanged. Given the location of legislator ideal points, $000' \; P \; 110 \; P \; 100 \; P \; 000'$; there is no undominated outcome. If the legislators cast optimal votes on each issue, the anticipated outcome is the bill (100). Confronted by that prospect, there is an incentive to form a coalition, $\{123\} \in W$, to support the bill as amended (110). Consistent with Theorem 1, each member benefits from this coalition: $110 \in v(1)$, $v(2)$, $v(3)$, but $110 \notin v(123)$. In this example, Theorem 2 implies minimal size since $100 \; P_i \; 110$, $i = 4,5$.

A second change in the original example also results in an essential game. Assume that the status quo is in its initial location, and that now $x = 111$, the bill with an amendment to the amendment. In this case each other outcome is preferred by a majority to the status quo; however, because $100 \; P \; 111 \; P \; 110 \; P \; 100$, the core is empty. Inspection of Figure 3 indicates that individual issue voting results in the collective choice of the bill with the first amendment (110). Thus, there is an incentive to form coalition $\{345\} \in W$ to support the bill as twice amended (111). Again, coalition formation is rational because $110 \in v(345)$, and minimal size follows

Thus it is possible to identify conditions related to the number of issues on the agenda and the outcome of legislative voting which affect the likelihood of an empty core. Because an empty core is necessary for rational coalition formation and size effects, these conditions provide a basis for several hypotheses concerning the outcomes of roll-call voting. In general, if the core of legislative voting decisions can be empty, then on average the number of votes recorded on the winning side should be less than if there is no possibility of an empty core.

Figure 3.



I_1	I_2	I_3	I_4	I_5
110	110	000′	100	100
100	000′	110	000′	000′
000′	100	100	110	110

Table 1 partitions roll-call outcomes in terms of the number of issues on the agenda and the outcome of voting. In each cell μ_i represents the mean number of votes cast on the winning side. Because a status quo outcome implies an inessential game, the first hypothesis is stated as follows:

H_1: $\mu_1 = \mu_2 = \mu_3$.

Single issue decisions also imply the existence of an inessential game:

H_2: $\mu_1 = \mu_4$.

If there are two issues on the agenda, there is at least the possibility of an essential game:

H_3: $\mu_5 < \mu_4$.

Finally, since the probability of existence of an essential game is increased by the presence of more than two issues on the agenda, there is likewise an increased probability of coalition size effects:

H_4: $\mu_6 < \mu_5$.

To test these hypotheses, roll-call data were taken from the U.S. Senate for the first session of the 97th Congress (1981). Of the 483 roll calls reported by the *Congressional Quarterly,* 467 are included in this analysis. Exclusions are limited to

Table 1. Mean Size of Roll-Call Winners

	Number of Issues on the Agenda		
Outcome	One	Two	More than Two
Status quo	μ_1	μ_2	μ_3
Other	μ_4	μ_5	μ_6

cases of special majority rule and incomplete decisions (no vote on final passage).

The results appear in Table 2 and contain a few surprises. The most obvious is an almost complete absence of status-quo outcomes. This makes it impossible to test the first two hypotheses. Such data as there are appear to contradict H_2, but n_1 is too small to support any conclusion. The third hypothesis is tested and apparently confirmed with a statistical difference of $t = 14.624$. Unfortunately, a large number of procedural decisions are included in $\bar{X}_4$, and consequently the difference between $\bar{X}_4$ and $\bar{X}_5$ may result from reasons other than coalition formation. The difference between $\bar{X}_5$ and $\bar{X}_6$ is substantively more convincing, but statistically marginal ($t = 1.652$). Thus, the data confirm H_4, but do not eliminate the alternative explanation that more controversial decisions entail more issues and closer outcomes.

Discussion

The data do not contradict the hypotheses regarding coalition-size effects, but do little more to support them; this may result from legislator failure to minimize coalition membership, even when there are gains to be made. However, there is an alternative explanation: Legislators may rarely form coalitions. Further consideration of the data and the spatial model lend some credibility to this view.

The most striking feature of the data is the implied success of legislation once it reaches the Senate floor. It may be amended to a greater or lesser degree, but it does not lose. This is partially because at present a good deal of legislative activity is focused on departmental authorizations and appropriations that must be passed eventually.

A second explanation follows from the congressional norm, "If it works, don't fix it"; that is, issues selected for legislative action are likely to be consistent with the spatial model shown in Figure 3. If the status quo is located some distance away from the distribution of voter ideal points, there is implied agreement that, in respect to a particular policy area, something ought to be done. Clearly disagreement may prevail in respect to the ideal policy, but the status quo is not included in the dispute. This too would contribute to the dearth of status-quo outcomes.

If for all practical purposes the status quo is not an option, three issue decisions are necessary to the existence of an essential game. Then H_4 would be a test for coalition-size effects, but not H_3.

The spatial analysis provides further reasons why legislative processes may be far less suscepti-

Table 2. Mean Size of Roll-Call Winners

	Number of Issues on the Agenda		
Outcome	One	Two	More than Two
Status quo			
	$\bar{X}_1 = 62.500$		
s	11.54		
	s = 4	0	0
Other			
	$\bar{X}_4 = 88.929$	$\bar{X}_5 = 66.360$	$\bar{X}_6 = 63.805$
s	9.223	14.260	15.920
n	70	178	215

ble to coalition formation than is ordinarily believed. Consider Figure 3 once more, with the status quo in its initial location. The question of the existence of an empty core is then focused on outcomes 100, 110, and $x = 111$. There are two collective preference patterns among these outcomes which yield an essential game: 1) 111 *P* 110 *P* 100 *P* 111, and 2) 111 *P* 100 *P* 110 *P* 111. The first is illustrated by the configuration of outcomes in Figure 3; the second would be realized if the spatial location of 110 and 111 were reversed.

In the first case note that creation of an essential game is a result of introducing the amendment to the amendment (111), which is preferred to the bill as once amended (110), but not the bill with no amendments (100). Now assume that the amendment to the amendment is introduced by a legislator who prefers 111 to 110, that is, I_3, I_4, or I_5. It is not at all obvious that any one of those legislators would choose to locate the second amendment as shown in Figure 3. Each of them has an incentive to select a location closer to the segment 3-5 than 111 and in the intersection of the shaded and crosshatched spaces. An amendment so located is preferred by all members of $\{345\} \epsilon W$ to 011 and 111, and unlike 111 it is also preferred by a majority to the bill (100). This eliminates the essential game, as well as the decision costs and possible uncertainty associated with coalition formation.

To consider the second case, assume (contrary to the notational convention) that 111 represents the bill with one amendment, and 110 represents the bill as twice amended. Again with the status quo at a distance, the essential game does not exist until the second amendment is introduced. This action would presumably come from either I_1 or I_2 who prefer 110 to 111, except that both have good reason to reject it. In the first place, the bill as twice amended (110) is not preferred by a majority to the bill as amended (111). Of greater interest, note that the bill as amended (111) will not win against the bill (100). Thus, if I_1 and I_2 are interested in an outcome closer to their ideal points, a better strategy is to let the first amendment go down to defeat and then introduce their amendment located at 110. This outcome is preferred by a majority to the bill and avoids creation of an essential game.

Systematic analysis of legislator incentives to introduce amendments, and especially those that create essential games, is beyond the scope of this article. The two examples make an interesting case, but do not support any obvious generalizations. Nevertheless, they do show that preference conditions exist under which legislators appear to have an incentive to avoid creating an agenda of the sort necessary for rational coalition formation. Thus, one may conjecture that opportunities for coalition formation are even less frequent than would result from random preference distributions.

Conclusion

In this article it has been proved that only essential ordinal preference games provide an incentive to form coalitions, that there is no incentive to increase the membership of a winning coalition, and that there may be an incentive to reduce the membership of a winning coalition.

These results do not depend on the assumption of transferable utility; they follow instead from the definition of ordinal preference games, including the set function, v, which determines from individual preferences a listing of the set of worst outcomes, $v(S)$, that can befall each $S \subseteq N$. The function is based on a with-in coalition unanimity rule; that is, a set of players may form a coalition only if all members agree that there is at least one outcome that is preferable to the anticipated outcome of a collective choice. In the absence of constitutional provisions, it is not assumed that any subset of the members of a potential winning coalition can coerce or otherwise induce the remaining members to accept an outcome that the latter consider inferior to the anticipated outcome. This underlines the effect of eliminating the transferable utility assumption. A favored subset of members of a winning coalition cannot distribute a portion of their gains to recruit supporters from those who would otherwise be losers. This line of reasoning is directly relevant to political decision making, where policy choices are among indivisible public goods as opposed to divisible private goods.

Application of the ordinal preference model to the question of coalition formation clearly weakens the size principle: Minimal winning coalitions are probable, but not certain. However, the extent to which Riker's result is weakened depends on the structure of collective choice. If winning coalitions are formed by adding players to subsets of insufficient size to win, the original size principle is preserved. It is only under collective choice structures where coalitions of greater than minimal winning size exist from the outset that the incentive to eject members in excess of the number necessary to win is uncertain. Again, this is consistent with political choice, because an excess of members on the winning side of a public policy decision does not necessarily affect the payoff to the remaining members.

The weakening of the size principle is countered by increased applicability. In numerous analyses of political coalition formation, the question of whether transferable utility is a plausible assumption is simply not considered.[9] Clearly, analysis based on strict regard for that condition would eliminate most political applications. The less restrictive assumption of ordinal preferences, which implies no side payments, supports a conclusion that the tendency to form minimal winning coalitions is a general characteristic of political choice processes.

The demonstration of a relationship between the configuration of individual preferences and the incentive to form a coalition provides refined criteria for predicting the occurrence of size effects. Theorem 1 proves that an empty core is the threshold condition on preference heterogeneity necessary to support rational coalition formation. Then, because the probability of gain from limiting coalition membership increases with preference heterogeneity, different institutions, issues, or both, can be expected to support different coalition-size hypotheses. Thus, one may conclude that studies of coalition size in such groups as courts, cabinet governments, and strategic alliances, gain credibility from the elimination of transferable utility. Further, the empty core condition implies, for example, court decisions limited to a single issue are less likely to exhibit size effects than cabinet formation decisions, which entail trade-offs on multiple issues.

The empirical results identify several reasons why previous tests for coalition-size effects using roll-call data have met with limited success. The prevalence of few-issue decisions assures the frequent presence of an outcome in the core. The finding that the status quo rarely wins implies that the existence of a core is even more likely than might otherwise be expected; if the status quo is generally not a feasible outcome of legislative voting decisions, three or more issue decisions are necessary for the existence of an empty core. The data show that for the 97th Senate (first session) more than 50% of the roll calls occurred on decisions with fewer than three issues.

Several factors may have contributed to the marginal success of the test for coalition-size effects. On three-issue decisions (excluding the status quo from consideration), a random distribution of voter preferences will produce an empty core in less than 10% of the cases. In the absence of obvious reasons for positing a greater relative frequency, this would reduce the observable size effects between two- and three-issue roll-call outcomes. This measurement problem is exacerbated to the extent that legislators, without regard for coalition formation, position issues as closely as possible to their ideal points while retaining the support of a bare majority of their colleagues.

The existence of a core, a deterrent to rational coalition formation, does not derive exclusively from the structure of legislative decision making. It may also result from a legislator's decision to add an issue to the agenda. McKelvey (1976) showed that agenda formation may be highly unstable, which at a minimum implies an empty core. His reasoning is based on the assumption that a single individual controls the agenda and thereby can assure the majority choice of his ideal point. However, in many legislative processes agenda formation is decentralized; each member has the option to introduce a bill or amendment. In that case, an individual may choose to locate his amendment at a point which only approaches his ideal point, but which is also stable. If legislators seek to locate issues that induce a stable social choice, the chance of observing coalition size effects is further diminished. Thus, an important question is the specification of conditions under which legislators have an incentive to form agendas that include an outcome in the core.

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[9] In 12 empirical papers on political coalition formation in Groennings, Kelley, and Leiserson (1970), there is at most infrequent reference to the game theoretic assumptions. Further, in the introduction the editors outline a set of "definitions which the contributors agreed to work with in drafting their chapters." These include reference to the number of actors (three or more), a "limited payoff" condition, and that coalition formation must be "explicit." Transferable utility is not mentioned.

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Liberalism, Civic Humanism, and the Case of Adam Smith

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In this article, I examine critically Donald Winch's interpretation of the politics of Adam Smith. I explain how Winch wrests Smith's political thought out of the larger vision of commercial society that is found in his moral, political, and economic writings, and how Winch misreads Smith's understanding of particular political problems such as the dehumanized workforce and the standing army. I also show how Winch's civic humanist reading of Smith's political thought fails to appreciate Smith's liberal conceptualizations of corruption and public-mindedness in a modern commercial society. Finally, I suggest that our failure to understand the politics of Adam Smith does not lie in our liberal interpretation of his work, as Winch claims, but in our understanding of what constitutes liberal political discourse.

The concepts of virtue and corruption have played a major role in the history of political thought since the ancient Greeks. Traditionally, histories of political thought have associated the decline of these concepts with the rise of liberalism in the seventeenth, eighteenth, and nineteenth centuries, and Marxism in the nineteenth and twentieth centuries (Cropsey, 1957; Cumming, 1969; Sabine, 1961; Wolin, 1960). A growing interest in the economic forces underlying political change is seen to displace gradually classical attempts to view politics through moral categories, and to give rise to a modern mechanistic view of the political order.

Over the past 15 years, a number of important attempts have been made to reinterpret seventeenth-, eighteenth-, and nineteenth-century thought through a reading of the republican or civic humanist tradition (Bailyn, 1967; Kramnick, 1968; Pocock, 1973, 1975a, b; Robbins, 1959; Wood, 1972). Focusing upon the central role played by the concepts of virtue and corruption in the republican tradition, these studies have led to a striking revisionist understanding of an evolving Anglo-American tradition of political discourse. Among the most prominent of the revisionist interpretations has been the work of J. G. A. Pocock.

According to Pocock, there emerged in post-1688 England a purely secular form of social criticism that drew heavily upon the civic humanist tradition of renaissance Florence in order to understand the impact of an expanding commercial economy upon English political life. Rejecting or minimizing the role of the individual in the marketplace, or of the merchant class in the economy, this civic humanism was based upon a perception of changes in the role of the individual as citizen, and in the autonomous citizen's ability to perform certain political and military duties. The function of property in the civic humanist paradigm was to insure the autonomy of each individual citizen in the performance of one's duties. Property, particularly landed or real property, was perceived to be the foundation upon which an individual's civic personality and civic virtue rested.

Pocock's interpretation has culminated in a serious reevaluation of the importance of liberal modes of thought in the seventeenth and eighteenth centuries. For Pocock, civic humanist writers were "the first intellectuals on record to express an entirely secular awareness of social and economic changes going on in their society, and to say specifically that these changes affected both their values and their modes of perceiving reality" (1975b, p. 461). Civic humanists did not investigate the dynamics of the marketplace nor did they accept the assumptions of "possessive individualism." They analyzed the modes of property which left men unduly dependent upon others from the property-personality-civic autonomy matrix. To the degree to which emerging social types such as the monied interest could not conform to these

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criteria, civic humanists denounced them as undermining or corrupting the social and economic foundations upon which the polity rested. To the degree to which these social types could conform to these criteria, civic humanists championed them as forces promoting virtue and stabilizing the political order.[1]

In *Adam Smith's Politics,* Donald Winch has written "an essay in historical revision" that draws heavily upon the civic humanist interpretation of eighteenth-century thought. By placing Smith in the context of civic humanism, Winch seeks to show how Smith's political views were affected by attitudes that were not liberal-capitalist in orientation and to explain why Smith's politics were not simply an episode "that occurred some way along a road which runs from Locke to Marx" (1978, p. 180). Far from representing the cutting edge of a liberal vision of the world, the politics of Adam Smith were part of the evolving tradition of seventeenth- and eighteenth-century civic humanism.

In this article I examine critically the Winch interpretation of Adam Smith's political thought. I argue that Winch's civic humanist reading of Adam Smith's politics has led to a serious misunderstanding of Smith's thought. Winch wrests Smith's political thought out of the larger vision of commercial society that is found in his moral, political, and economic writings. As a result, Winch fails to appreciate the degree to which Smith's understanding of particular political problems, such as a dehumanized workforce or the standing army, exists not only outside the logic of civic humanist thought, but in direct opposition to it. I also explain why Winch's analysis fails to appreciate Smith's "liberal" conceptualizations of the problem of corruption and public-mindedness in a modern commercial society and how these, too, stand outside the orbit of seventeenth- and eighteenth-century civic humanism. Smith's vision of commercial society, his understanding of what holds it together, and his insight into what threatens to undermine it are a far cry from seventeenth- and eighteenth-century civic humanist thought. Our failure to understand the politics of Adam Smith does not lie, as Winch claims, in our liberal interpretation of his work, but in our understanding of what constitutes liberal political discourse.

Winch's Smith

At the heart of Winch's reading of Adam Smith's politics lies an attempt to recover a lost political dimension to Smith's thought. According to Winch (1978, p. 26), "Smith employs a consistent method or style of political analysis in his writings and lectures which cannot readily be encompassed within the categories of the liberal capitalist perspective." In order to understand this lost dimension "it is necessary to bear in mind that he is frequently employing a well-established public language for discussing such matters—a language, the resonances of which were already well known to the educated members of his immediate audience" (1978, p. 5). Surprisingly, Winch's explanation of exactly what this "well-established public language" encompasses is rather vague. Constant references to the work of J. G. A. Pocock and his ongoing attempt to read Smithian political ideas in light of the property-personality equation found in the civic humanist tradition leave little doubt that the "public language" Winch is referring to is civic humanism. Indeed, chapters 4-7 demonstrate a systematic attempt to read Smith's analysis of particular political questions of the day, such as the question of the relationship between commerce and liberty, the problem of a dehumanized workforce, the public debt issue, and the standing-army controversy, in terms of the ideas of contemporary civic humanist writers.

The problem with Winch's civic humanist reading of Smith's politics is that it ultimately fails to come to terms with the broader world view that structures Smith's analysis of particular political problems of the day. This world view does not work out of the assumptions or the logic of the traditional civic humanist language of discourse, nor does it reflect a suspicious outlook toward the modern commercial order as is found in the work of most civic humanists. Indeed, the Smithian world view both accepts and champions the modern commercial order. The difference between Smith's perception of commercial capitalism and that found in civic humanism can be best appreciated by looking briefly at two aspects of Smith's thought: his analysis of the rise of commercial societies in Book III of *The Wealth of Nations,* and his use of the "four-stage theory" in Book IV.

Of Commerce and Liberty

There is little doubt that Smith draws heavily upon the historical arguments developed by David

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[1] Kramnick (1982) recently has suggested that the civic humanist interpretation has gone too far in its attempt to read Lockean liberalism out of eighteenth-century Anglo-American political thought. Macpherson (1962), Meek (1967, 1977), and Hirschman (1977) also provide alternative interpretations of the development of political and economic ideas in eighteenth-century Anglo-American political thought.

Hume in the essays "Of Commerce" and "Of Refinement in the Arts" as well as in *The History of England.* Smith himself credits Hume with being the first modern writer to understand the relationship between commerce and liberty (1965, p. 385). For Winch, the Humean connection puts Smith's thought solidly in the civic humanist tradition because "Hume's language and mode of conducting his argument . . . can also be described in terms of the civic humanist perspective" (1978, p. 74).

There are a number of problems with this interpretation. First, it fails to appreciate the larger political impetus behind Hume's political and philosophical writings. Hume did not develop his historical arguments about the relationship between commerce and liberty in the modern world in order to criticize modern commercial society from within a civic humanist perspective. As Forbes (1975) has shown, he developed it to move contemporary thinking about politics beyond the narrow court-country debate within which civic humanist arguments thrived. Ultimately, Hume sought to explain why commerce tended to promote, rather than to undercut, liberty in the modern world.

A second problem with this interpretation is that it fails to appreciate how antithetical the notion of liberty found in Hume's historical writings and in Smith's economic writings is to the civic humanist mind. Both Hume and Smith agree with their civic humanist contemporaries that feudalism had undercut liberty by fostering conditions of economic and social dependency. But the liberty that Hume and Smith believe accompany the "order and good government" introduced by commerce and manufactures is not that found in civic humanism. For the civic humanist, the idea of liberty is integrally connected to the idea of the independent citizen who is capable of engaging in politics and of acting in the public interest. Liberty thus is conceived in a highly positive manner along the lines of classical Greek thought. In contrast, the notion of liberty discussed by Hume and Smith is a mean, almost entirely negative liberty. Commerce and manufacture do not create the economic conditions that might enable citizens to engage in political activity. They only free individuals from the dependency found in the feudal system. Thus although the marketplace frees individuals from immediate personal servitude, it does not make them into autonomous, independent citizens.

The most disturbing problem with this interpretation is its failure to understand the role that this anti-civic humanist line of argument played in the larger line of argument developed in *The Wealth of Nations.* Smith's analysis of the relationship between commerce and liberty is not developed primarily to move beyond civic humanist thought, but to explain historically why, despite earlier successes, the mercantile system posed a serious threat to future economic growth. To appreciate the political import of the historical arguments developed by Smith in Book III of *The Wealth of Nations,* they must be read in light of his larger economic arguments and his critique of the mercantile system.

Book I of *The Wealth of Nations* centers around a discussion of the role played by the division of labor in promoting economic growth. Book II focuses on the role played by the accumulation and use of capital. Two ideas lie at the heart of Smith's capital theory. First, Smith asserts that the accumulation of capital is a necessary precondition to the division of labor. As the division of labor advances, an increasing amount of capital must be accumulated beforehand in order to provide workers with the equipment and materials necessary for production (1965, pp. 259-260). Second, he maintains that there is a distinction between productive and nonproductive labor. "The annual produce of the land and labour of any nation can be increased in its value by no other means, but by increasing either the number of its productive labourers, or the productive powers of those labourers who had before been employed" (1965, p. 326). In other words, capital expended on nonproductive activities is considered to be wasted capital, at least in terms of promoting economic growth and increasing the wealth of the nation.

Book II concludes with a discussion of why capital can be used more fruitfully in one sector of the economy than in another in light of these two ideas. Smith notes that capital employed in the retail trade can support those productive shopkeepers and tradesmen who sell directly to the consumer. In their profits "consists the whole value which its employment adds to the annual produce of the land and labour of the society" (1965, p. 343). Capital can also be used to promote the transportation of crude and manufactured products from where they abound to where they are needed. Employing capital in this manner not only directly supports the productive activities of wholesale merchants, but also indirectly puts into motion the labor of the farmer and the manufacturer. "Its operation in both these respects is a good deal superior to that of the capital of the retailer" (1965, p. 344). Capital also can be used in the manufacture and preparation of raw materials and produce for immediate use and consumption, which puts into motion an even greater quantity of productive labor than does an equal amount in the hands of a wholesale merchant. Finally, Smith notes that capital can be employed in obtaining the crude produce needed for the use and consumption of society. "The capital employed in agriculture, therefore, not only puts into motion a greater quantity of productive labour than any equal capital employed in manufactures, but in proportion too to the quantity of productive labour which it employs, it adds a much greater value to the annual produce of the land and labour of the country, to the real wealth and revenue of its inhabitants." Capital used in agriculture was considered to be "by far the most advantageous to the society" (1965, p. 345).

Smith's economic understanding of the place of agriculture in a commercial society clearly lies outside the civic humanist matrix of property-personality-virtue. Agriculture is important not because it is the foundation upon which civic virtue rests, but because it is the optimal sector for allocating capital. This point is reemphasized in chapter 1 of Book III of *The Wealth of Nations,* "Of the Natural Progress of Opulence," where Smith describes optimal capital allocation under conditions of natural liberty.

According to Smith, a man will naturally prefer to employ his capital in the improvement and cultivation of land, rather than in trade or manufacture, given equal or nearly equal profits. "The man who employs his capital in land, has it more under his view and command, and his fortune is much less liable to accidents, than that of the trader, who is obliged frequently to commit it, not only to the winds and the waves, but to the more uncertain elements of human folly and injustice, by giving great credits in distant countries to men, whose character and situation he can seldom be thoroughly acquainted" (1965, p. 358). Similarly, given equal or near equal profits, a man will naturally prefer to employ capital in domestic manufacturing than in foreign commerce. As Smith explains, "As the capital of the landlord or farmer is more secure than that of the manufacturer, so the capital of the manufacturer, being at all times more within his view and command, is more secure than that of the foreign merchant" (1965, p. 359). In other words, according to a "natural progress of opulence," capital is directed in a growing commercial society first to agriculture, then to manufacture, and finally to foreign commerce. "This order of things is so very natural, that in every society that had any territory, it has always, I believe, been in some degree observed" (1965, p. 360).

It is within the context of this discussion of the natural progress of the opulence that Smith takes up the historical arguments considered by Hume. Although Smith believes that economic growth must take place to some degree according to the natural progress of opulence, he observes that in all modern European states the "natural order of things" has become "in many respects, entirely inverted." Modern Europeans often find it more to their advantage to employ their capital in distant foreign commerce, even though much of the land in their own country remains underdeveloped or uncultivated. In addition, he points out that manufacture and commerce were responsible for giving birth to the major improvements in agriculture in Europe. In contrast to Hume, Smith thus is not interested primarily in explaining how commerce had promoted, rather than undermined, liberty in the modern world. Instead, he wants to explain what circumstances forced European states "necessarily . . . into this unnatural and retrograde order" (1965, p. 360). For Smith, this boils down to understanding how some political arrangements stifled natural economic growth, whereas others promoted a seemingly unnatural form.

The "economic" problematic that underlies Smith's historical arguments is clearly evident in three of the four chapters of Book III. In chapter 2 Smith explains how the feudal institutions of primogeniture and entails were responsible for undermining the natural progress of opulence from the fall of Rome to the modern commercial era. Because the great feudal landlords simply lacked the ability or the incentive to cultivate and improve the land, stimulus for economic growth in the countryside ultimately had to come from some sector of the economy that had escaped the restrictions of feudal laws and custom. In chapter 3 he describes how towns and cities gradually freed themselves from the political rule of the feudal landlords. Finally, in chapter 4 he discusses the impact that independent town life had upon European economic development.

For Smith, unlike Hume, the significance of the rise and development of commerce out of independent town life does not lie simply in the political fact that it introduced good government and liberty into the modern world; it lies in the economic fact that the modern mercantile order had developed contrary to the natural order of things. In the past, foreign commerce had been the leading sector of economic growth and had been responsible for stimulating growth in all other sectors of the economy. As Smith's capital theory shows, however, foreign commerce was also the least productive and most unstable sector of the economy. As long as the nation continued to rely upon foreign commerce as it had in the past, future economic prosperity would rest upon the most unstable of foundations.[2]

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[2] Smith writes, "The capital, however, that is acquired to any country by commerce and manufactures, is all a very precarious and uncertain possession, till some part of it has been secured and realised in the cultivation and

In sum, Smith's discussion of the rise and development of commercial society out of the feudal system and his use of Humean historical arguments about the relationship between commerce and liberty lie outside the rhetoric of civic humanist critics of commercial society. Smith's arguments culminate not with a rejection of commercial society but with an appeal to throw off the mercantilist policies that continued to force the British economy into an "unnatural and retrograde order." Similarly, the call for a "system of natural liberty" is not a rejection of the modern commercial order and the economic growth that was an essential part of it; it is an affirmation of the commercial order and a call to place future economic growth on more secure foundations.

The Four-Stage Theory

Winch's misinterpretation of the politics of Adam Smith is not limited to his analysis of Smith's account of the rise of commercial societies in Book III of *The Wealth of Nations.* He also fails to appreciate fully how Smith's analyses of particular problems were shaped by the so-called four-stage theory, or how this theory set apart Smith's political ideas from the thought of other civic humanist writers. This failure is particularly evident in Winch's analysis of Smith's contribution to the standing army controversy (1978, chap. 5).

For most modern readers, Smith's inquiry into the nature and expense of military defense in the different stages of society seems rather inconsequential and parochial. In the context of eighteenth-century England, however, he was addressing one of the most sensitive political issues of the day—the question of the standing army. For civic humanist pamphleteers throughout the eighteenth century, the existence of the standing army was perceived to be a constant threat to civil liberty; it reflected the corruption introduced into the body politic by commerce. Only by maintaining a watchful citizen militia, they argued, was it possible to secure the nation from those forces that threatened it both from within and without (Pocock, 1973, chap. 4; Winch, 1978, chap. 5).

Like other contemporary civic humanist writers such as Adam Ferguson and John Millar, Smith was willing to concede that there were basically two methods by which a modern commercial society could protect itself from outside invasion. Either it could attempt to instill a martial spirit into the entire population "by means of a very rigorous police," an alternatiave supported by Ferguson, or it could "render the trade of a soldier a particular trade, separate and distinct from all the others" (1965, pp. 658-660). The first option, Smith notes, went against the interests as well as the natural inclinations of those who live in a commercial society. No matter how disciplined such a force might become, it would always be inferior to a well-disciplined and well-trained military force. His position in regard to the standing-army controversy thus pits him directly against contemporary civic humanists. Only by means of a professional standing army could a commercial society protect itself from the violence and injustice of other states.

Winch concedes that Smith's position on the standing-army controversy places him at odds with contemporary civic humanist thought, but he nevertheless maintains that the setting and the premises on which he poses the problem leave Smith in the civic humanist matrix. In particular, Winch notes that the discussion takes place in the context of a moral and social analysis of the consequences of the division of labor in modern societies and reflects a civic humanist concern over the preconditions for effective citizenship (1978, p. 113). Unfortunately, at least for Winch's argument, Smith himself does not use the republican notion of citizenship in his analysis of commercial society. Indeed, it will be argued below that such a concept, as well as the accompanying civic humanist notions of virtue and corruption, are foreign to his understanding of the modern commercial order. Moreover, his discussion of the social and moral consequences of the division of labor is actually part of a larger argument, the so-called four-stage theory, that places his analysis of the standing-army controversy firmly outside the civic humanist worldview.

Smith's four-stage theory originates with his contention in both the unpublished *Lectures on Justice, Police, Revenue and Arms* and Book V of *The Wealth of Nations* that there are four distinct stages to human society: hunting, pasturage, farming, and commerce. In categorizing each stage of human history by the mode of subsistence that predominates in it, Smith is not trying to make a Marxian-type argument that these are the actual stages of western European historical development. Instead, he is trying to formulate a

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improvement of its lands. A merchant, it has been said very properly, is not necessarily the citizen of any particular country. It is in a great measure indifferent to him from what place he carries on his trade; and a very trifling disgust will make him remove his capital, and together with it all the industry which it supports, from one country to another. No part of it can be said to belong to any particular country, till it has been spread as it were over the face of that country, either in buildings, or in the lasting improvement of lands" (1965, p. 395). For a discussion of Smith's capital theory, see Bowley (1975) and Hollander (1973).

developmental perspective for understanding how and why different stages of economic development give rise to different sorts of political arrangements and what the proper role of political institutions should be in the modern commercial order (Höpfl, 1978; Skinner, 1967, 1975).

Significantly, this general line of argument bears little resemblance to the cyclical notions of time and economic change found in seventeenth- and eighteenth-century civic humanist thought.[3] On the contrary, it represents an extension of the natural histories of economic improvement and societal development found in the work of Locke, Mandeville, and Hume. Much like the natural histories of these earlier liberal writers, Smith's four-stage theory is used to explain why political institutions in a commercial society were necessarily different from those in earlier stages of society, and what the proper functions of these institutions were, given the social, political, and economic problems brought on by economic development.[4]

The way in which this liberal four-stage theory shapes Smith's perception of the standing-army controversy is delineated at the beginning of Book V of *The Wealth of Nations,* where he considers the duties that remain for a sovereign under the system of natural liberty. "The first duty of the sovereign," writes Smith, "that of protecting the society from the violence and invasion of other independent societies, can be performed only by means of a military force. But the expence both of preparing this military force in time of peace, and of employing it in time of war, is very different in the different stages of society, in the different periods of improvement" (1965, p. 653).

According to Smith, in a nation of hunters, every man is a warrior as well as a hunter. When a hunter goes to war, he maintains himself by his own labor, much as he would during times of peace. As a result, a hunter society "is at no sort of expence either to prepare him for the field, or to maintain him while he is in it" (1965, p. 653). The military situation in more advanced countries differs considerably from that found in the more primitive hunting societies. Shepherd societies have no fixed place of habitation, and thus, unlike hunter societies, are not completely limited by the bounty of nature. Although an army of hunters might be limited to 200 or 300 men because of their precarious mode of subsistence, an army of shepherds could potentially number in the thousands. A nation of shepherds thus is a much more formidable military opponent than a nation of hunters (1965, pp. 653-654).

The military situation found in more advanced farming societies is different still. In a farming society having little foreign commerce and no manufacture, every man remains a warrior or easily becomes one. The hardiness of ordinary life in a farming society prepares individuals physically and mentally for the rigors of war. As long as military expeditions begin after seed-time and end before harvest, an individual is able to participate in military affairs with little or no cost to himself. As in hunting and shepherd societies, the expense of war falls on the warriors themselves, not upon the sovereign (1965, p. 655). In terms of Smith's four-stage theory, the age of the husbandmen is effectively the age of the civic humanist citizen militia.

Three factors intervene in modern commercial societies to undermine the viability of citizen militias. First, the progress of manufactures makes it all but impossible for an individual who goes to war to maintain himself at his own expense. Whereas in an earlier stage of society a man could take to the field without seriously disturbing his source of income, in a commercial age a worker who is compelled to quit his place of work also loses his sole source of income. Similarly, farmers have little extra time for warfare, given the improvements that the progress of manufactures introduces into the countryside. As a result, "Military exercises come to be as much neglected by the inhabitants of the country as by those in the town, and the great body of the people becomes altogether unwarlike" (1965, p. 659). Second, improvements in the art of war make it one of the most intricate and complicated arts in a commercial society. In order to master this art it becomes necessary for the citizen to spend an increasing amount of time in military exercises. Third, the wealth that follows from the economic improvements found in commercial societies provokes the invasion of all their neighbors who desire their wealth. "An industrious, and upon that account a wealthy nation," Smith thus concludes, "is of all nations the most likely to be attacked; and unless the state takes some new measures for the public defence, the natural habits of the people render them altogether incapable of defending themselves" (1965, p. 659).

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[3] By noting that two important republican writers of the day, Adam Ferguson and John Millar, made arguments that paralleled those developed by Smith in his four-stage theory, Winch appears to argue that Smith therefore employs a civic humanist line of argument in his political thought. However, quite the contrary was the case. By grounding some of their arguments in a four-stage theory argument, both Ferguson and Millar were introducing tensions into their own thought that could not be easily reconciled with their republican values.

[4] It is important to recognize that the four-stage theory is not purely a product of the liberal tradition. The earliest modern versions of a stage-theory argument are found more frequently in continental rather than British writers. Moreover, Ronald Meek has argued that the four-stage theory is actually a pre-Marxist rather than a liberal theory. (See Cumming, 1969; Höpfl, 1978; Meek, 1967, 1977; Skinner, 1967, 1975.)

By considering the question of the standing army in terms of the four-stage theory, Smith thus was able to account for the fact that at a certain age in societal development citizen militias could protect the nation from external threats. In addition, it enabled him to explain why in a commercial society such an option was no longer viable. In Smith's mind, a standing army had to be maintained by a commercial society if it were to "be perpetuated, or even preserved for any considerable time" (1965, p. 667). Far from revealing civic humanist leanings, the four-stage theory demonstrates how far out of touch with the problems of a modern commercial society Smith views civic humanist thought to be.

Corruption in Commercial Society

Two questions remain to be considered: What notions of corruption and public-mindedness does one find in Smith's work? How do these notions differ from those found in the civic humanist tradition? It is beyond the scope of this article to provide a comprehensive examination of the moral theory found in *A Theory of Moral Sentiments* (1976) or the way in which this moral theory complements and reinforces the economic arguments found in *The Wealth of Nations.* These issues have been dealt with at great length in other works on Smith (see Campbell, 1971; Cropsey, 1957; Lindgren, 1973; Morrow, 1969). However, we can briefly explain why Smith's notions of corruption and public-mindedness stand outside a civic humanist vision of the commercial order and what these show about a Smithian understanding of the problems confronting a modern commercial society.

One of the most striking differences between Smith's understanding of corruption and public-mindedness and that of contemporary civic humanist writers can be seen in the distinct underlying concerns. As noted earlier, civic humanists were concerned with the economic preconditions for independent action as a citizen. Landed property was perceived to be the foundation upon which an individual's civic personality was based because it enabled an individual to act autonomously and virtuously in public affairs. Significantly, Smith's discussion does not center around an analysis of the economic preconditions to effective citizenship, but around a discussion of the psychological ties that bind men together in all societies.

According to Smith, every independent state is divided into a number of different orders, each with its own particular privileges, powers, and immunities. The constitution of a particular state is determined by the orders found in a society and the distribution of powers, privileges, and immunities found among them (1976, pp. 376-377). Every individual is naturally, that is psychlogically, more attached to one's own particular order than to any other and seeks to expand and defend that order's privileges and immunities from the encroachments of other orders. This partiality is not necessarily bad, because it provides an important psychological check upon undesirable social changes. As Smith notes, "It tends to preserve whatever is the established balance among the different orders and societies into which the state is divided; and while it sometimes appears to obstruct some alterations of government which may be fashionable and popular at the time, it contributes in reality to the stability and permanency of the whole system" (1976, p. 377).

Along a similar line of argument, Smith maintains that the ties that ultimately bind these different orders together into a common political community, like those that tie individuals to their own order in society, are also derived from the moral sentiments. Indeed, Smith maintains that the distinction of ranks and the order of society are ultimately founded upon the disposition of mankind to go along with the passions of the rich and the powerful (1976, pp. 114-116). As Smith explains,

> That kings are the servants of the people, to be obeyed, resisted, deposed, or punished, as the public conveniency may require, is the doctrine of reason and philosophy; but it is not the doctrine of nature. Nature would teach us to submit to them for their own sake, to tremble and bow down before their exalted station, to regard their smile as a reward sufficient to compensate any services, and to dread their displeasure, though no other evil were to follow from it, as the severest of all mortifications. (1976, p. 116)

The political community found in a commercial society thus does not emerge out of the activities of economically independent citizens. It arises out of the natural disposition in individuals to defer to others in positions of authority.

A tone of uncertainty and anguish underlies Smith's discussion of the psychological phenomena found in all human societies that stands in contrast to the moderately upbeat and self-confident tone found in civic humanism. Smith's ideas reflect a world view that appears to be unsure of itself and of its future. His concern is not to create better citizens who can participate in the political life of the community, nor is it to reform the existing power structure. It is to maintain those conditions most highly conducive to the continuance of existing authority relations. The world of Adam Smith is not the civic humanist world analyzed by Pocock nor is it the world of nineteenth- and twentieth-century mass democracy. It is the troubled world of early liberalism sketched out by Wolin in *Politics and Vision* (1960, chap. 9). Nothing makes this more evident than Smith's conceptualization of the problems introduced into the commercial order by the division of labor.

Smith fully recognizes the devastating impact that the division of labor could have upon the work force. A man who spent his whole life performing a few simple operations naturally loses his ability to exercise his mind "and generally becomes as stupid and ignorant as it is possible for a human creature to become." As Smith explains,

> The torpor of his mind renders him, not only incapable of relishing or bearing a part in any rational conversation, but of conceiving any generous, noble, or tender sentiment, and consequently of forming any just judgment concerning many even of the ordinary duties of private life. Of the great and extensive interest of his country he is altogether incapable of judging; . . . The uniformity of his stationary life . . . corrupts even the activity of his body, and renders him incapable of exerting his strength with vigour and perseverance, in any other employment than that to which he has been bred. His dexterity at his own particular trade seems, in this manner, to be acquired at the expense of his intellectual, social, and martial virtues. But in every improved and civilized society this is the state into which the labouring poor, that is, the great body of the people, must necessarily fall, unless government takes some pains to prevent it." (1965, pp. 734-735)

In Winch's mind, this discussion of the mental mutilation of the work force brought on by the division of labor is further evidence of the humanistic side to Smith's politics. This reading simply fails to understand the concerns lying behind the discussion. Smith's concern is not with making workers into responsible citizens in the republican sense of the term. His four-stage theory had shown how foreign to a modern commercial society such a notion of citizenship was. In a commercial society, workers could not be expected to perform the military duties assumed by republican-minded citizens in farming societies (see Smith, 1965, pp. 658-659). Moreover, as the above quotation shows, Smith simply does not believe that a modern worker is capable of participating rationally in the political world. His concern is that the division of labor might dehumanize the worker to such a degree that the moral sentiments themselves might become corrupted and that the psychological bonds that tie the various orders in a society together might be torn asunder. Consequently, his solution to the problem of the division of labor, public education, is not aimed at creating independent citizens in the republican sense of the term; rather it is concerned with preventing "the almost entire corruption and degeneracy of the great body of the people" (1965, p. 734). This concern over the corruption of the moral sentiments of the people is shown clearly in a fascinating passage from Book V of *The Wealth of Nations.*

> Though the state was to derive no advantage from the instruction of the inferior ranks of people, it would still deserve its attention that they should not be altogether uninstructed. The state, however, derives no inconsiderable advantage from their instruction. The more they are instructed, the less liable they are to the delusions of enthusiasm and superstition, which, among ignorant nations, frequently occasion the most dreadful disorders. An instructed and intelligent people besides, are always more decent and orderly than an ignorant and stupid one. They feel themselves, each individually more respectable, and more likely to obtain the respect of their lawful superiors. . . . They are more disposed to examine, and more capable of seeing through the interested complaints of faction and sedition, and they are, upon that account, less apt to be misled into any wanton or unnecessary opposition to the measures of government. In free countries, where the safety of government depends very much upon the favorable judgment which the people may form of its conduct, it must surely be of the highest importance that they should not be disposed to judge rashly or capriciously concerning it. (1965, p. 740)

In short, the notion of corruption found in Smith's discussion is qualitatively different from that found in the civic humanist tradition. Rather than emerging out of a concern over the economic preconditions for the independent action on the part of the citizen, or centering around the property-personality-civic virtue paradigm traced out by Pocock, Smith's notion of corruption emerges out of his concern for the psychological attachments which tie men in different social orders together into a common political community and make authority relations possible.

Public-mindedness

Like his analysis of corruption in a commercial society, Smith's discussion of the problem of getting people to act in the public interest exists far outside the civic humanist world view. For the civic humanist, this problem was solved by creating "virtuous men" who had the economic independence to be able to transcend their private interests and to look upon the good of the community as a whole. Such a solution was unavailable to Smith. Smith recognizes that in a commercial society, individuals stood in constant need of the cooperation and assistance of others. One could not reasonably expect to gain this through either friendship or benevolence. Commercial society was simply too large and diverse for such bonds of affection to tie individuals together for any period of time. Indeed, Smith notes that an individual would be much more likely to gain the assistance of others by appealing to self-interest. "It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest. We address ourselves, not to their humanity but to their self-love, and never talk to them of our own necessities but of their advantages" (1965, p. 14).[5]

For Smith, the problem of public-mindedness had to be dealt with in terms of the self-interest of individuals, not in spite of it. His concern was not to discuss how political actors could transcend their self-interest, but how this self-interest ultimately was tied to the public interest. When does the self-interest of an individual coincide with the public interest? When does it not? What factors might motivate political actors to seek out the public interest while acting in their own interest? These are the questions that propel his thought forward. His answers do not lie outside his economic theory, but are firmly embedded in it.

All too often, the underlying political objective of Smith's economic theory in *The Wealth of Nations* is forgotten by commentators. Smith was not writing an abstract treatise on economics that was to be read solely by academic scribes. He was addressing a particular order—the landlords in parliament—with a particular political objective in mind: to teach them what their economic interests were in a commercial society. Smith recognizes that of the three great orders in a commercial society, the landlords, the laborers, and the mercantile-manufacturers, only the latter had acquired a true understanding of what their specific interests were in matters of economic policy. Laborers had neither the time nor the necessary education or habits to think about public affairs in any systematic manner. The landlords, on the other hand, often suffered from an indolence "which is the natural effect of the ease and security of their situation" and which left them "not only ignorant but incapable of that application of mind which is necessary in order to foresee and understand the consequences of any public regulation" (1965, pp. 248-259).

The consequences of this state of affairs were devastating. By a superior knowledge of their own interest, the mercantile-manufacturer order had been able to impose its will upon the generosity of the "country gentleman" in parliament by convincing him that their interest, and not his or the laborers, was the interest of the public. As Smith warns, "The interests of the dealers, however, in any particular branch of trade or manufactures, is always in some respects different from, and even opposite to, that of the public" (1965, p. 250).

The economic theory developed in Book I of *The Wealth of Nations* is meant to provide the landlord order with an understanding of why the interest of the mercantile-manufacturer order differed from the general interest of society and why the interest of the landlord order was "strictly and inseparably" connected with it. In providing the members of the landlord order with an understanding of their own class-interest in economic affairs, Smith also was trying to teach them how to act in the long-run interest of the community as a whole. As Smith concludes at the end of Book I, "When the public deliberates concerning any regulation of commerce or police, the proprietors of land never can mislead it, with a view to promote the interest of their own particular order; at least, if they have any tolerable knowledge of that interest" (1965, p. 249). For Smith, economic theory thus held the key to getting people to act in the public interest. By teaching them what their own interest was in economic matters, those country gentlemen could also be pushed to act in the public interest.

Interestingly, Smith does not believe that economic theory rightly understood need appeal solely to policymakers' naked self-interest. It could also appeal to their love of system and their regard for the beauty of order, of art, and of contrivance; that is, to their moral sentiments. Smith writes,

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[5] Smith does note in *A Theory of Moral Sentiments*, "The wise and virtuous man is at all times willing that his own private interest should be sacrificed to the public interest of his own particular order or society. He is at all times willing, too, that the interest of this order or society should be sacrificed to the greater interest of the state or sovereignty of which it is only a subordinate part" (1976, p. 384). The notion of virtue discussed here, however, is a private-personal virtue, not a public one. In neither *The Wealth of Nations* nor in *A Theory of Moral Sentiments* does he argue that the solution to the problem of public-mindedness in a commercial society lies primarily in the creation of virtuous men. Kramnick argues that a new liberal notion of virtue emerged in eighteenth-century political thought that was tied into the idea of the self-centered individual who participated in the market (1982, pp. 663-664).

> The perfection of police, the extension of trade and manufactures, are noble and magnificent objects. The contemplation of them pleases us, and we are interested in whatever can tend to advance them. They make part of the great system of government, and the wheels of the political machine seem to move with more harmony and ease by means of them. We take pleasure in beholding the perfection of so beautiful and so grand a system, and we are uneasy till we remove any obstruction that can in the least disturb or encumber the regularity of its motions. (1976, p. 305)

If a policymaker correctly understands the forces that move a modern commercial economy forward, he will be "animated to some degree of public spirit" and will feel some desire to remove those obstructions that prevent "so beautiful and so orderly a machine" from working properly. As Smith explains,

> Nothing tends so much to promote public spirit as the study of politics,—of the several systems of civil government, their advantages and disadvantages,—of the constitutions of our own country, its situation, and interest with regard to foreign nations, its commerce, its defense, the disadvantages it labours under, the dangers to which it may be exposed, how to remove the one and how to guard against the other. Upon this account political disquisitions, if just, and reasonable, and practicable, are of all the works of speculation, the most useful. Even the weakest and the worst of them are not altogether without their utility. They serve at least to animate the public passions of men, and rouse them to seek out the means of promoting the happiness of the society. (1976, p. 307)

In sum, for Smith the problem of instilling a concern over the public interest into the political order was not one that involved maintaining the economic preconditions for the independent action of an autonomous citizen. It was the establishment of the correct psychological disposition in the minds of policymakers to promote policies that served the public interest. The notion of public-mindedness found in Smith's work, like that of corruption, thus is quite different from that found in civic humanism. It is a liberal public-mindedness that accepts and, at times, champions, the self-interest that lies at the heart of modern commercial society. It is a liberal public-mindedness that is integrally related to liberal economic theory.

Conclusion

I have examined critically Donald Winch's civic humanist reading of Adam Smith's politics. My ongoing concern has been to explain why Smith's political thought should not be read apart from his economic thought or the larger conceptual framework within which his perception of commercial society operates. I do not mean to imply that conventional treatments of Smith's political thought fully appreciate the political dimension to Smith's thought. Winch is quite correct when he argues that far too little attention is paid to Smith's concern over the problems of corruption and public-mindedness in the modern commercial order. Unfortunately, Winch fails to see how closely connected to Smith's economic theory proper his understanding of these problems are. For Smith, economic theory is an important mode of political discourse that is integrally related to his political thought and his perception of the problems confronting the modern commercial order. Our failure to understand Smith as a political thinker does not reside simply in our failure to appreciate the "political" dimensions to his thought, but in our failure to understand how economic discourse itself shaped his perception of the modern commercial order.[6]

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[6] Political theorists' general failure to appreciate the political implications of liberal economic thought is not limited to the case of Adam Smith. For example, the actual economic writings of John Locke have all but been ignored by political theorists. For a discussion of the relationship between Lockean political and economic thought see Harpham (in press) and Vaughn (1980).

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Heidegger on Freedom and Community: Some Political Implications of His Early Thought

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Despite their widespread influence, Martin Heidegger's works have rarely been assessed for their own direct significance as political theory. In this article I undertake such an assessment by drawing out the political implications of Heidegger's understanding of freedom and community in two of his early works. Specifically, I argue that, although Heidegger's thought has been interpreted as advocating political conservatism, it in fact propounds a new kind of radicalism which is neither precisely conservative nor progressive, although decidedly revolutionary. When this is brought to light, it is easier to see the connection between Heidegger's works and the earlier Philosophy of Freedom, as well as ways in which they anticipate important trends in contemporary political thought.

Martin Heidegger's thought has had a deep and widespread influence on the political theory of the twentieth century. This influence ranges from the effect on Marxism that Heideggerian ideas have achieved through the works of Kojeve, Lukacs, Marcuse, and Sartre to their manifold contribution to hermeneutics and structuralism.[1] Nevertheless, some 50 years after the publication of *Being and Time,* Heidegger's works have rarely been assessed for their own direct significance as political theory (Harries, 1978, p. 306).[2] This is due in part to a certain interpretation of Heidegger's thought which stresses the importance of purely personal freedom as against the wholly "inauthentic" norms of society. But as I show, and as most interpreters agree, Heidegger is anything but a radical individualist—that is, anything but apolitical in the strict sense. A certain kind of sociality is, for Heidegger, constitutive of human life at the most basic, ontological level, of man as he is apart from any particular regime, society, or culture.[3]

Another reason for the relative neglect of the political implications of Heidegger's thought stems from his involvement with the National Socialist regime in Germany during 1933. This episode has invited many interpretations, ranging from those who regard it as a personal aberration with no intrinsic connection to Heidegger's thought (Arendt, 1978; Krell, 1977, pp. 26-28; Waehlens, 1946; Weil, 1946) to those who see a connection, more or less direct, between Heidegger's political activity and his works of the period (Adorno, 1973; Harries, 1978; Lowith, 1946; Rosen, 1969, pp. 119-124; Steiner, 1978, pp. 109-121).

It is not the purpose of this article to explore this issue directly, which has been done at some length elsewhere. But it needs to be stressed that, just as it would be an error to separate entirely Heidegger's political activity from his thought, it would be an equally great mistake to believe that

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[1] For an overview of Heidegger's influence on political theory, see Steiner (1978, pp. 12, 74, 105, 140-143) and Jay (1973, p. 71-72, 122, 272).

[2] The major studies are Poggeler (1972), Schwan (1965), and Palmier (1968). There is no general study of Heidegger's political thought in English-language scholarship comparable to, for example, Shlomo Avineri's volumes on Marx and Hegel. Blitz (1981) limits himself to *Being and Time* and, in pursuit of his major theme, leaves aside Heidegger's connection with earlier German philosophy (see his remark, p. 20).

[3] Schmitt (1969, pp. 250-252) and Nicholson (1971, p. 713) believe the political teaching of *Being and Time* to be an anarchistic one. But as Harries points out, this involves a selective reading which ignores Heidegger's insistence that "man exists essentially with others" and shares "the destiny of a people" (1978, p. 311-312). Hoy writes similarly of Heidegger's insistence that "Dasein (man) is not an isolated, private ego but most primordially a social, communal and historical being" (1978, pp. 329, 339-341).

the political significance of Heidegger's thinking was exhausted by its connection with National Socialism.[4] As I show, the political implications of Heidegger's philosophy of the 1920s and early 1930s reach back into the nineteenth-century tradition of the Philosophy of Freedom and at the same time anticipate trends in political, social, and critical theory that are of major importance today. I will have something to say about Heidegger's understanding of what, even in 1935, he called "the inner truth and greatness" of National Socialism, but less to reopen the debate over his actions and motives during that era than to show what kind of political expectations and commitment can be seen to emerge from an internal analysis of his thought.

Among the more notable political interpretations of Heidegger's early thought is Adorno's claim that it culminates in a conservative willingness to leave the world as it is, so that "what man is anyway once more becomes his goal" (1973, pp. 36-37). In my view, this is a misunderstanding of a call to political commitment which cannot be characterized as either conservative or progressive in the usual senses. Heidegger's understanding of freedom as "seizing upon" the "destiny" we share with our "community" and "people" demands, as we shall see, their most thoroughgoing transformation, a recommitment to our "heritage" which is paradoxically, as Heidegger says, "against it and yet for it" (1957, pp. 383-386).[5] My analysis is drawn from *Being and Time* and *An Introduction to Metaphysics.* The former remains Heidegger's single most influential work. The latter recommends itself both as a commentary on and elaboration of some ideas central to *Being and Time* and as containing some of Heidegger's most candid assertions about twentieth-century politics. No single article could exhaust the political implications of these works, much less of all of Heidegger's writings. By concentrating on Heidegger's understanding of freedom and community as presented by them, I hope to draw out the political bearing of his early thought in its clearest and most forthright meaning.[6]

Fundamental Ontology: Heidegger's New Approach

According to Heidegger, wherever we encounter human beings, they are inextricably involved with a "world" that they share with "others." This "everyday" life is, however, profoundly "alienating." In order to remedy this alienation, man must achieve "authenticity" through "freedom-toward-death" (1957, pp. 16-17, 178, 266). Both the theoretical demonstration of this freedom and the guide to its actual exercise, lie in what Heidegger calls "fundamental ontology," which he regarded as an entirely new approach to the understanding of human existence. Fundamental ontology is in turn rooted in the central and most daunting aspect of Heidegger's thought as a whole, the "Question of Being." Some preliminary consideration of these concepts must be given before we can trace their broader political implications.

For Heidegger no less than for Hegel, Being must be understood in terms of the origin, the generative process from which things emerge, not as a realm of eternal ends or substance in which human nature seeks its perfection and has a permanent place. Instead, according to Heidegger, man experiences Being or life as an "overpowering" force. It is a *polemos* (the ancient Greek word for war) in which man is engaged as a kind of chief warrior, struggling to make his home in the matrix of history. Like all other things, man is generated out of this overpowering force. But he alone is capable of turning back against it to achieve his "being-free" (1953, pp. 46-49, 160-63). In the richest, fullest sense of his existence, man *is* this dynamic moment of struggle; not a fixed entity, but a *way* of living. Thus fundamental ontology does not deal with what man is according to some conception of his permanent nature, but *how* he becomes what he is in any particular historical epoch (as Heidegger puts it in a famous dictum, "Higher than actuality stands *possibility*" (1957, p. 38)). Its aim is to uncover the set of active conditions by which a world is generated in the struggle of man with Being.

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[4] As Nicholson remarks, a third way must be found of addressing the political implications of Heidegger's thought besides the two extremes of characterizing it as "fascist" or "unpolitical" (1971, pp. 708-710).

[5] The page references for *Sein and Zeit* are to the German edition (Heidegger, 1957) and are included in the margins of Macquarrie's and Robinson's English translation (Heidegger, 1962). The page references for *Einfuhrung in die Metaphysik* are to the German edition (Heidegger, 1953), with the corresponding pages from Mannheim's English translation (Heidegger, 1975) following the semicolon.

[6] For the purposes of this discussion, "Heidegger's early thought" means until roughly 1935. Some interpreters have found a turn in Heidegger's thought away from *Being and Time,* whereas others see a continuity between it and the later works. Heidegger himself discouraged the notion of a turn in his works. See the discussion in Krell (1977, pp. 24-26, 29-35). Harries aptly describes *An Introduction to Metaphysics* as "developing new themes" whose "roots" are in *Being and Time* (1978, p. 306). My concern here is limited to showing that *An Introduction to Metaphysics* elaborates concepts from *Being and Time* in a way that is politically more illustrative because it is a politically more engaged work.

Heidegger maintains that every historical world as it actually exists—every nation with its specific politics, culture, and morality—has these "ontological foundations" as its origin and basis (1953, pp. 16-17, 50). Before seeing how he claims to be able to uncover these foundations, we must note an important feature of Heidegger's argument. Heidegger is at pains to emphasize that these underlying foundations—or, as we might term them, structures of possibility—do not imply a universal definition of human behavior. They imply, he says, neither the "primitiveness" of a universal anthropology (a kind of Rousseauistic state of nature) nor a universal ideal not yet achieved (1957, pp. 50-51). For in his view, ontologically speaking, there is neither a nature nor a "world history" of man. There are only the unique and independent worlds that have grown, as it were, out of local encounters with Being (led, as he says in *An Introduction to Metaphysics,* by rulers and poets of the first rank (1953, pp. 117, 152)). In other words, man has no pure freedom beyond the particular world to which he is "committed," and his commitment takes place nowhere except in the midst of this world, its people, and their heritage. This heritage, a people's "ownmost distinctive possibility," discloses the fixed possibilities within which human beings move. Their future will also move within these possibilities (1957, pp. 20-22, 128, 383-384).

Fundamental ontology may therefore be termed an abstraction from every possible world. It explains only the most general way in which worlds come into being, and nothing concrete about what they become—French, German, Russian, or American. These considerations help to explain the notoriously abstract quality of Heidegger's many neologisms (such as "being-with-one-another-in-the-world"). For the very abstractness of fundamental ontology is necessary so as not to impinge upon the unique "destiny" (1957, p. 56) which shapes each world and people from its origins with false universalizations about substantive human nature or historical progress. In this sense, Heidegger stands in principle against the universalism both of classical political philosophy with its notion of a timeless human nature and of Hegelian and Marxist notions of history. Like Nietzsche, whose precedent he came increasingly to ponder, Heidegger tries to elaborate a *theory* of particularity, of the minimal conditions for mutually exclusive horizons. Bearing this in mind, let us see how Heidegger thinks that the primordial struggle with Being emerges in the very midst of everyday life at its most tranquil and settled.

Man in the World, Freedom, and Authenticity

According to Heidegger, we are not individual subjects placed in a world like one object placed into another, such as apples into a bowl. Rather we are already "there" in the grip of a world—"thrown" into, "delivered over," and "submitted to" a world which we must "answer to." Moreover, our access to this world is always co-experienced with others. "Being-in" a world and "being-with" others are inseparable and irreducible (as Heidegger says, "equiprimordial") dimensions of the original moment of struggle to build a home in the midst of history (1957, pp. 54-55, 135, 162). For Heidegger this original and unspecified sociality is the condition for the possibility of all concrete relationships (1957, pp. 57, 118-120).[7]

However, Heidegger argues, when we encounter human beings as they are "first of all and most of the time," in ordinary "everyday" circumstances, they have forgotten their involvement with others in the momentous origins of the world, because that world has assumed an appearance of permanence and stability (1957, p. 16). This bewitching appearance "tempts" us to regard the world as a finished product (1957, p. 177). We stand passively before it, mindful only of how it might be preserved as it is. The need to get along as best we can in a world for which we no longer bear any active responsibility alienates us from others and, since we are involved with others at the very core of our being, alienates us from ourselves. We grow "unsociable" as the "primordial" relationship of "being-with" is covered over, well-nigh obliterated, by what Heidegger calls the "they-self" (1957, p. 125).

Heidegger's evocation of the "dictatorship of the 'they,' " a dictatorship that includes, but goes beyond the seemingly self-perpetuating and disembodied mechanism of public opinion, has made a powerful contribution to the sociology of alienation from bourgeois "mass" society (Steiner, 1978, p. 140). For, despite the abstract quality of Heidegger's presentation, it points clearly to what has been widely perceived as the hectic self-interest, hypocrisy and loneliness of twentieth-century individualism. The they-self (*das Man-Selbst*) is a creature of isolated "ones" which reinforces its members' fear of failing to conform to the world as it now exists by impersonally prescribing what "one" (*man*) may or may not do.[8] The they-self's desire to manage and fortify the world's power to remain as it is serves to "tranquilize" human behavior and prevent a reemergence of those risks that lie concealed at the origins of the world (1957, pp. 126-129).

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[7] As Sartre comments: "The empirical image which may best symbolize Heidegger's intuition is not that of a conflict but rather a *crew.* The original relation of the Other to my consciousness is not the *you* and *me*; it is the *we*" (1966, pp. 300-303).

In the they-self's world, genuine communication is "dimmed down" to an inconsequential and mediocre "chatter" (1957, pp. 138, 167, 194-195). Specifically, the people's "heritage" is dimmed down, forgotten, or treated as irrelevant in order to obscure any heterodox values that might spring out of the past to challenge the illusion of a perfectly self-sufficient, timeless and ongoing present (1957, p. 21). The politics of everyday life is thus reduced, as Heidegger puts it, to a utilitarian "reckoning and balancing of claims" among egoistic individuals (1957, p. 283). In this connection, Heidegger derides the social welfare programs of modern states and the public morality of "empathy" which they promote. These conventional links of "empathy" (sustained, presumably, by legislation and public opinion) take for granted the existence of fundamentally isolated individuals who have already come to view one another as static, self-contained objects like the everyday world altogether (1957, pp. 121-125, 130).

In his boldest departure, Heidegger argues that reason itself as it has hitherto been understood is a creature of the everyday world. The impending triumph of global technology which came increasingly to occupy Heidegger's thought must, he claims, be understood on the basis of a "wrong turn" first taken by Plato. This wrong turn consists in forgetting the war-like process of struggle between man and Being out of which all things emerge to take on a stable "presence" (*Anwesenheit*), and instead to elevate this objective aspect of things into their sole and absolute reality. We treat things as static, self-contained objects because we view them in the "reflected light" of everyday life in its full-blown appearance of performance (1957, pp. 25-26, 165-165, 172, 225).

Heidegger's sweeping assimilation of the history of philosophy into an alienating, objectifying stance toward the world better enables us to understand the insistently nonempirical—critics would say nonscientific, even irrational (Carnap, 1978)—character of fundamental ontology. The problem, in his view, with the usual human or social sciences (*Geisteswissenschaften*) such as political science, psychology, and anthropology, is that they start by observing the everyday world in its appearance of objective permanence and then devise methodologies to organize these observations. In other words, the social sciences presuppose a particular, independent world without realizing that these observations are "true" for this world alone. The "ontological foundations" of all such worlds can never be approached through "hypotheses derived from empirical material," since these hypotheses can never be valid for any world other than the one from which they are derived—a world that is unique and therefore substantively incomparable with other worlds. The social sciences, having been elaborated in terms of the empirical "actuality" of the everyday world, are incapable of reaching behind and uncovering the grounds of their own "possibility" (1957, p. 16, 45-50).

To be a captive of the they-self is to live "inauthentically." But—and this is a crucial point—to live authentically, or, more literally, to become *real* (*eigentlich*), in no way means to transcend or withdraw from the world from which everyday life has emerged. For everyday life is merely the "dimmed down," fossilized and devitalized form of an original encounter with Being. The "temptation" to live inauthentically is thus "constitutive" of man in the depths of his "being-there" (*Dasein*). It is neither a "sin" which can be redeemed, nor a " 'fall' " from a natural or Biblical golden age (1957, pp. 175-177, 273). There is, in short, no escape from one's world, its people and heritage. To live authentically, therefore, consists in "seizing upon" the vital origins, the struggle out of which the everyday world has issued, and *reenacting* those origins (1957, p. 179). Since there is no escape from the brute "facticity," the contingency, of the particular world into which we have been thrown, "commitment" can take no other form than self-consciously "choosing to make this choice"; "coming back" to the "definite possibilities" of this one world beneath the encrusted "actuality" of everyday life (1957, pp. 144, 268).

The alienation that characterizes everyday life at its most typical and fully developed acts as the spur to this coming back. For Heidegger argues that the empty routines of everyday life, the frantic busyness and chatter that achieve nothing real, steal over us in a mood of profound "anxiety" (1957, pp. 188-189). Subverting our mere "fear" (*Furcht*) of failing to conform and get alone, this anxiety bespeaks a sneaking consciousness of everyday life's groundlessness—its vaporous, "rootless" quality as it floats further and further from the "house" (*Heim*) of our primordial interaction with Being. Anxiety dispels the illusion that we have been "living concretely." On the contrary, we feel "uncanny" (or "unhoused"—*unheimlich*), "floating" and "fallen" in the midst of the familiar and customary (1957, pp. 175-180).

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[8] In German, *Man* means "they"; in English *man* means the impersonal "one."

This very anxiety is a disguised blessing because it is an intimation of the underlying impermanence and mutability of Being. Our intimation of the necessary fragility of all attempts to build a permanently satisfying world brings us face to face again with that struggle with life's "overpowering" challenge to build a world. In order to reenact the origins of our world, we must be liberated from the they-self's delusion that the everyday world can remain as it is forever. We can "choose to make this choice" only when we recognize that it is the impossibility of our choice lasting forever that makes it possible and necessary to choose again. Anxiety reveals, at bottom, the consciousness of our finitude. We are creatures of "care" (*Sorge*), burdened with and subject to the passing away of time, because we are bounded by death. Man exists authentically when he "anticipates" and "resolves upon" this finitude. "Resolve" establishes our "freedom-towards-death," the freedom to face everyday life's impermanence and thereby to face the need to reestablish the world out of which everyday life has grown (1957, pp. 266, 307-308).

The Authentic Political Community

The broader political implications of this argument emerge in Division 2, chap. 5 of *Being and Time.* Heidegger reiterates that the resolve to face our finitude always involves the choice we have already made with others as a "community" (*Gemeinschaft*) and a "people" (*Volk*). This choice, as he puts it, guides our "fates" (*Schicksal*) as individuals "in advance" as the "destiny" that comes down to us as the community's unique historical existence. "Destiny" is profoundly collective. It is no mere aggregate "put together . . . out of individual fates," since individuality itself is but a one-sided abstraction from the relationships of "being-in" the world and "being-with" others (1957, p. 384).

Confronting the finitude, arbitrariness and particularity of our world dispels the complacency of the they-self, enabling us to see ourselves for what we really are. The distractions and superficial diversity which mask the emptiness of everyday life collapse like a shell. This resolve upon the primordial origins is, however, just as much a blind sally into the future. For it involves the rejection of everything customary and familiar, of all 'inauthentic everydayness," as we "take over" the "basis" of our world and set its destiny back into motion. Heidegger, figuratively describing this return to the origins as going "under the eyes of Death" (1957, p. 382), seems to mean that a community's character becomes clearest when its finitude is most nakedly evident, when it is shaken to its foundations by the possibility of destruction. Although he does not provide an illustration here, he might say, for instance, that in times of war, nations hearken back to the tradition of their founding—ordinarily paid lip service or ignored—for new inspiration, in order to reaffirm what they really stand for. This return to the origins can act as a clarion call to shake off the complacency of present-day life in order to face a dangerous, uncertain future.

A commentator on this chapter of *Being and Time* has noted its "portentous and threatening overtones" (Steiner, 1978, p. 109). Given this vague yet ominous atmosphere, the reader must wonder: What concrete form would a people's recommitment to its destiny take? What exactly *is* the community's "ownmost distinctive possibility"? But Heidegger believes he can offer no guidance about such concrete goals—"what Dasein *factually* resolves" as it takes over its "factical basis" (1957, p. 383). This is consistent with the abstractness of fundamental ontology, an abstractness which, as I observed earlier, is required to preserve the uniqueness of every world. But by paying attention to the details of Heidegger's argument, it is possible to infer several things with which this recommitment cannot be identified.

First, it cannot be limited to the mere reform of existing conditions. Ordinary political controversies about improving institutions, morality, culture, or the distribution of wealth are not radical enough in Heidegger's view, since the very familarity of the disputes and their objects keep us chained to the everyday "present." In keeping with his views on social welfare measures noted earlier, we can surmise that for Heidegger incremental reform would represent the most superficial tinkering with a fundamentally unreal way of life. Any success it might achieve would be far outweighed by the danger that the success itself would perpetuate the illusion of the all-around adequacy of the they-self's world. Heidegger's disdain for any compromise with the conditions of ordinary political dispute (with what contemporary, and especially Anglo-American readers, would tend to identify altogether with politics) is manifest in his dismissal of the whole questions of legal and distributive justice as a "mere reckoning and balancing of claims," a part of that "everyday chatter" by which the they-self manages its affairs.

Given the total absence of politics as usual from Heidegger's understanding of an authentic political community, it bears emphasizing that he does have a definite kind of political action in mind. In the section of *Being and Time* under consideration, he is at pains to stress that fundamental ontology does not merely provide a framework for "speculating" about resolve, but a guide to action, for "really coming back to one's factical 'there' " (1957, p. 383). Each people must "seize upon" its origins "for its 'time' " and "in its 'generation' " (1957, p. 385). No one can "extricate himself" from this recurring responsibility. As for Heidegger's view of his own role as a thinker in this "coming back," this is made explicit in *An Introduction to Metaphysics.* Here, philosophy is presented as a kind of partisan activity on behalf of a people. It "breaks the paths and opens the perspectives of . . . the knowledge in and by which a people fulfills itself historically and culturally" (1953, pp. 8, 10). This is in keeping with the argument earlier noted that philosophy has no privileged universal standpoint that could enable it to hold itself aloof from the call of the local "destiny." Rather, like every other human activity, philosophy achieves authenticity only when it joins the community's self-recovery. But it has a special contribution to make. Man's questioning and doubt about his world are, as Heidegger puts it, "pre-ontological" (1957, p. 12), a vague but strong feeling of anxiety that creeps into everyday life. The special task of fundamental ontology is to raise this feeling to consciousness, to make it clear and explicit. Another way of putting this is to say that if the truth of human experience lies in its primordial origins, as variable as place and fortune, rather than in a substantial end or a universal ideal, that truth can in no way transcend one's own "people" but must reside in their world organically.

In *An Introduction to Metaphysics* it is clear, too, that the "struggle" (*Kampf*) of man with Being is meant to be taken quite literally. In one of Heidegger's rare concrete descriptions of "primordial" political activity, we learn that the resolve to recover the greatness of a people's origins cannot exclude violence (1953, pp. 115-116, 130-131, 149-151, 171). The ancient Greek *polis* is commended as an example of a place where history "happens" because there are real rulers (1953, pp. 117, 152-153). Real rulers are "violent men" who "use power" (*Macht*) in a reflection of Being's own "overpowering" force, which they take the lead in confronting. There is a parallel line of reasoning in the section from *Being and Time* under consideration, where our freedom-towards-death is said to reveal our "powerlessness" before the "power" of destiny. This self-abandonment to the community's destiny, by providing an unambiguous and unavoidable direction for commitment, paradoxically confers on us the "superior power" of single-minded and purposive action (1957, pp. 384-385).[9] In describing the real ruler, Heidegger admonishes us not to overlook the creative importance of violence because of the common, disparaging use of the word. This judgment, he argues, stems from merely conventional standards of everyday compromise and peacekeeping.

It is clear, in light of the foregoing, that Heidegger's political teaching is a revolutionary one. But it is equally clear that the community's recommitment to its destiny cannot embrace revolutionary *progress* in either the Hegelian or Marxist sense. For Hegel and Marx, man is transformed progressively and cumulatively through the objective conditions of political, cultural and economic life in which he is currently engaged. The pursuit of freedom is shaped by the result of past strivings and conditioned by the objects for overcoming offered by current historical existence. It is in this sense that both Hegel and Marx claimed an "empirical" basis for their theories of history, and rejected as romantic atavism any notion of a backward leap to the pristine origins (Hegel, 1956, pp. 10, 40-41; Marx, 1972, pp. 13, 53). But for Heidegger, there is no such dialectic of progress. The mediation of freedom, in the Hegelian or Marxist sense, by current empirical conditions would, from Heidegger's viewpoint, chain it to the inauthentic conditions of everyday life and the pervasive dictates of the they-self. (As we noted earlier, Heidegger criticizes the social sciences for being too bound up with the empirical content of everyday life to reveal the path to fundamental ontology.) For Heidegger, freedom must be seen, rather, as the return to a protean, indeterminate nothingness which overturns all existing conditions without developing them or being developed by them.[10]

[9] Man's self-abandonment to the power of destiny, in which he takes onto himself the power of destiny by becoming, so to speak, its conscious agent, is not to be confused with subjective willfulness. By subjective willfulness I mean the assumption, which Heidegger takes to be characteristic of everyday life, of a dichotomy between the wills of individual human subjects and the objective world. In the everyday world, man exerts his will in order to control and manage this objective environment through, for example, economic production. For Heidegger, the exertion of an egoistic will (or a group of such wills) against the objective environment presupposes the sway of "inauthentic everydayness." The subject/object dichotomy is thus the very opposite of the dynamic unity established by the welling up of destiny into authentic human existence, the reciprocal confrontation between man and Being.

[10] As Hoy remarks, "An important feature of Heidegger's ontological analysis of historicity is that nothing follows from the analylsis about what the *content* of his-

Perhaps, then, Heidegger means to encourage conservatism in the sense of bolstering cherished traditional ways of life, as Adorno's interpretation has it, sunk in "the homey murmuring" of custom (1973, p. 53).[11] This would seem to be the only remaining alternative. And yet Heidegger is most emphatic that this is not the kind of "repetition" of a people's heritage that he has in mind (1957, p. 383). For in his view, a tradition is inauthentic if it in any way buttresses everyday life. Traditional elements which are recognized by the they-self do buttress everyday life even if they seem at odds with its main tendencies, since they do constitute a part of its monolithic totality. Thus, the origins that a people must reenact stand across a chasm from everything familiar, whether it be a part of the present or of the commonly accepted interpretation of the past. "Repetition," he says, "does not let itself be persuaded of something by what is 'past,' just in order that this, as something which was formerly actual, may recur." Instead, "repetition is a *disavowal* (revocation or retraction—*Widerruf*) of the 'past' " (1957, pp. 383-386).

An Introduction to Metaphysics provides a concrete illustration of what this means. Speaking in 1935, Heidegger decries the "campaign against . . . today's intellectualism" on the ground that it is not sufficiently radical. By restricting itself to combatting recent intellectual culture, this campaign appears to justify those who advocate a proper use of the traditional intellect. In order to be truly effective, such a campaign, he implies, would have to uproot not only "today's intellectualism" but the "spiritual reaction" (*Reaktion*) which hopes to rescue a relatively more old-fashioned kind of intellectualism. He warns against this defense of what is presumably Germany's pre-Weimar intellectual culture as "the feeding ground of political (reaction)" (1953, pp. 93, 122). The kind of uprooting Heidegger has in mind is far more revolutionary than any such "mere restoration": "we have embarked on the great and long venture of demolishing a world grown old and of rebuilding it truly anew, i.e., historically" (1953, pp. 96, 125-126). Heidegger, then, embraces neither a conservative nor a progressive view of historical change. As he sums it up in *Being and Time,* "Repetition does not abandon itself to that which is past, nor does it aim at progress" (1957, p. 386).

For the above reasons, we must conclude that Heidegger's understanding of the community's recommitment to its destiny dissipates the distinction between radicalism and conservatism in their generally accepted senses—both, for him, components of inauthentic living, buttressing the they-self even when its "chatter" over conflicting political goals is at its most heated. One can sum up this deeply paradoxical teaching in the following way: The community's reassumption of its destiny requires a new kind of radicalism which is both backward- *and* forward-looking. It requires the rejection of all existing political, social, cultural, and moral bonds in the name of a contentless communitarianism; a wholly abstract notion of "the people" rooted in a past so primordial as to bear little if any resemblance to what is regarded in everyday life, by custom, public, and intellectual opinion, as the people's past history. This enables us to understand the meaning of the formula quoted at the beginning of this article: In order to be "for" our heritage, we have to go "against it." To resolve upon a past so totally unfamiliar would seem to require the most thoroughgoing futurism.[12]

Because the emptiness out of which the everyday world is projected is indeterminate and substanceless, the world that each people builds up in this void is, to stress an earlier point, unique and arbitrary. Man cannot, as Heidegger says, get back "behind" this void and fill it once and for all, nor can he transcend it (1957, p. 284). Authentic political life is grounded neither in nature nor in "world history." In the absence of any such guide for specific, limited goals, it would seem that the reenactment can only take on the character of audacious daring ("power," "struggle," "resolve," "violence"); a tremendous negative energy purging everyday life in the longing to reexperience what Heidegger calls the "moment of vision" when the community's world sprang arbitrarily into being (1957, p. 385). Consistent with his rejection of everyday life and the kind of

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tory must be. Hegel's concept of history, in contrast, is far more prescriptive" (1978, pp. 344-345).

[11] Adorno argues that because fundamental ontology is so abstract, it "succumbs to cultural mediations all the more; they recur as social aspects of that ontology's own purity." Hence, it becomes the hostage and legitimizer of the "here and now" (1973, pp. 2-21, 53, 92, 100, 109-113). Although Adorno is correct to point out the unmediated character of fundamental ontology in comparison with, for example, Hegelian or Marxist dialectic, he greatly underestimates the "revolutionary radicalism," to use Lowith's phrase (1946, p. 349), of Heideggerian resolve and repetition.

[12] The paradoxical—i.e., backward- and forward-looking, or conservative and radical—character of Heidegger's thought is reflected in the debate between Gadamer and Habermas over the proper use of hermeneutics as transmission of culture or critique of ideology (see Gadamer, 1977, pp. 29-36).

reason characteristic of it as standards for guiding the community's recommitment to its destiny, Heidegger cannot define authenticity in any concrete way: He can only evoke it negatively as "that . . . *by the neglect of which*" we are inauthentic (1957, p. 268). The authenticity of the community's recommitment is not, in other words, conditioned by the goal at which "resolve" is aimed, but by the *quality* (the boldness and passion) of the resolve—as Heidegger puts it, by the "totality . . . of one's resolute*ness*" (1957, p. 383—emphasis added).[13]

In concluding my analysis of Heidegger's idea of an authentic political community, I should look for a moment specifically at *An Introduction to Metaphysics.* This work, prepared from a series of lectures given in 1935, applies and develops the categories of *Being and Time* in accordance with Heidegger's understanding of the historical situation of his own "people" at that period. As such, it is a valuable supplement to *Being and Time,* which, as we have seen, is noteworthy for its lack of concreteness. To some extent, the later work shares this characteristic. Heidegger speaks of the tendency of all peoples at any given time to "fall" from their original encounter with Being into the dehumanizing conformity of everyday life. Yet the work bristles with contemporary references to "our people," "the revolution" and "our extraordinary tasks," some of which we have examined already. Among them, the reference to the "inner truth and greatness" of National Socialism in the encounter with "global technology" is the most notorious (1953, pp. 8, 29, 96, 152; 10, 38, 126, 199).[14]

The contrast in this regard between the two works can best be explained by the fact that Heidegger did not regard all peoples as being equally open to the summons back to the "greatness" of their origins. Moreover, he believed that our own time had reached a crisis point inasmuch as technology, the creature of everyday life at its most sophisticated and pervasive, virtually threatened to engulf the globe. He also believed that Germany had a special mission among "peoples" to halt this process and restore the possibility of authentic community for itself, Europe, and "the West" as a whole (1953, pp. 28-29, 34-35, 37-38, 45-46).

"Our people," he says, are caught between the "pincers" of America and Russia, which are dominated by "the same dreary technological frenzy, the same unrestricted organization of the average man." Germany, he goes on, must "wrest" its destiny from within itself, and thereby "move the history of the West" out of these pincers. Here is the idea, earlier discussed, that the prospect of destruction, by revealing the finitude of a people in all its precarious fragility, dispels the complacency of everyday life and convinces that people of the need to risk the recovery of its destiny. Expressed in *Being and Time* as one of the formal ways in which fundamental ontology acts as a guide "back to one's factical 'there,' " it comes to life here in what Heidegger sees as Germany's dire situation between the pincers of two powerful foes, who have raised inauthenticity to a new and unprecedented level. (For a discussion of these passages in their contemporary context, see Poggeler, 1974, pp. 29-31.)

A violent resoluteness to sweep away a world infected with the technological and managerial politics of modern times; the vision of a pure and unified community which will broach no compromise with ordinary political squabbling and interests; the conviction in the signal importance to the West as a whole that Germany lead this revolutionary reencounter with Being: On the basis of these considerations one could begin to examine the openness or vulnerability of Heidegger's philosophy to the kind of political alternative offered by National Socialism. The dismissal of all concrete political issues as a self-interested "reckoning and balancing of claims" could well induce one to view the boldest and least programmatic of political movements as the authentic voice of the community in its underlying, protean vitality. Moreover, if nothing transcends the historical community, and if such a movement is spreading everywhere, representing itself as the anti-Party aimed at sweeping away the selfish interests of both the traditional left *and* right, might one not embrace it as the legitimate harbinger of that destiny which is true for one's own people and it alone?

As suggested at the beginning, however, it would be a mistake to believe that the political significance of Heidegger's thought was exhausted by this connection. Now that I have shown some of its implications, it is possible to make some observations about the significance of these works for contemporary political theory.

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[13] Lowith (1946, p. 348) sees in Heideggerian resolve the "decisionist" philosophy of Carl Schmitt placed at the service of a unified and historically unique German community. Marx (1971, pp. 247-251) and Hoy (1978, pp. 343-344) observe that Heidegger's philosophy is incapable of providing criteria for choosing between morally good and bad courses of action. I think it should be added that, while setting no moral limits on resolve itself, Heidegger directs this resolve toward a definite kind of political alternative, if only by exclusion of other alternatives. Thus, as Blitz remarks, "authentic political resolve . . . would not be true of every political alternative" (1981, p. 217).

[14] For an exculpatory interpretation of this remark, see Schwan (1965, pp. 134-137); for a criticism of Schwan, see Harries (1978, pp. 323-324).

Conclusion

Heidegger's philosophy shares with the earlier Philosophy of Freedom a concern with the problem of how to achieve a cohesive community in a world increasingly dominated by the values of liberal individualism. Despite the many and considerable differences among them, Hegel, Marx, and Nietzsche shared a profound antipathy for the selfish materialism and vulgarity which the liberal political theorists of the Enlightenment had, in their view, elevated as the definition of human nature. Heidegger's strictures on the "they-self" and the "dreary technological frenzy and organization of the average" strike a chord with this long-standing critique. Heidegger is also in agreement with the Philosophy of Freedom that man's capacity for satisfaction does not reside in a permanent nature, but in the freedom to overcome the constraints of the present in cooperation with other free beings.

The demand for a creative, communitarian freedom still distinguishes continental political theory from the Anglo-American, liberal emphasis on fair opportunity and distribution. But between the Philosophy of Freedom and, for example, the Frankfurt School, there lies the chasm of an enormous problem. Earlier thinkers had believed that this community (however envisioned) would emerge *out of* bourgeois life; that the progress of history toward the bourgeois present had advanced man even as it had oppressed him. It was believed, moreover, that some harbinger for the superior freedom of the future was already emerging in the present, whether through the embodiment of the conditions for freedom in the present epoch (Hegel) or an emerging class of the soon-to-be free and equal (Marx). The radical political thought of the twentieth century takes shape, by contrast, against the background of severely diminished expectations that a fuller freedom was thus steadily and observably coming into being. Heidegger's philosophy of the twenties and thirties shares in this sentiment and attempts to bridge this chasm.

In this regard, note first of all the connection between Heidegger and Lukacs. As Goldman (1977) has shown, the two shared many perceptions in common of the crisis tendencies and pervasiveness of contemporary bourgeois values. Lukacs' category of reification, Goldman demonstrates, occupies a position in his theory analogous to Heidegger's category of "presence," the objective aspect of things whose elevation into absolute reality is the hallmark of everyday life. Both shared the conviction that the critique of modern society had to be deepened beyond the socioeconomic. As I have shown, Heidegger believed this was necessary because objective socioeconomic conditions (the everyday world) held no key for man's liberation from modern life; man was in no way progressively enriched and advanced by them. The belief that new human beings would emerge from such a progression, whether it be Marx's proletariat or even Nietzsche's superman, had, in his view, merely elevated and strengthened the grip of modern rational organization. Heidegger's response to this condition is to advance a notion of community so radical that, under its influence, alienation or lack of freedom can be seen not merely as material or even relative deprivation, but as the oppressive need to live in a world where anything is fixed and permanent. By a standard so pure, differences between modern "technological" regimes become negligible, so that Russia and America are, as Heidegger put it, "metaphysically the same" (1953, pp. 34-35, 45).

Following from this, Heidegger's theories have had two general kinds of influence on contemporary political thought. The first is a much-heightened sensitivity toward the perceived failings of modern society extended beyond the economic to include the cultural, psychological, and esthetic, accompanied by a lack of certainty that any currently existing class or group has a demonstrable historical mission to remedy them. This position, broadly characteristic of the Frankfurt School, has been described by Kolakowski as "Marxism without . . . the proletariat" (1981, p. 357).[15]

The other influence, exemplified by Lukacs, has been to help transform the notion of a socialist revolution from the maturation of a process already deep at work within the recesses of bourgeois society into a Promethean refashioning of all political and social existence *ex nihilo* by an elite of creators. As Alasdair MacIntyre has observed of Lukacs, despair over the prospect that modern society (including the working class) will generate the antidote to its own defects can make one vulnerable to expectations for a superman to arise on the strength of his own uncompromising vision and cleanse it from top to bottom. Although MacIntyre describes this tendency in Lukacs as Nietzschean (1981, p. 244), Lukacs's ideal party of creators, shattering the bonds of the reified world, might equally be thought to partake of Heideggerian "resolve." Certainly the despairing view of the prospect for an evolution out of

[15] See also pp. 20-21, 53, 92, 100, 109, 110-113.

twentieth-century society toward a fuller freedom has much in common with Heidegger.[16]

The political potency of Heidegger's philosophy might, I think, be summed up in this way. Like the earlier German thinkers, he rejects the conception of human nature in favor of an active, historical conception of man. At the same time, he frees the historical definition of man from any need to *demonstrate* itself in the concrete conditions of everyday life and politics. In other words, he attempts to reject the progressive notion of history without abandoning a historical definition of man. The radical cutting edge of the demand for freedom and community thus comes close to lopping away every restraint of circumstance, unloading the burden of empirical demonstrability which Hegel and Marx had believed essential to their notions of historical change. Fundamental ontology attempts to elucidate the permanent conditions of human impermanence or historicity. Such a standard, always underlying everyday life but never absorbed into it, can stand forth as a critique of everyday life that is recurrent, radically pure and beyond compromise. These characteristics place Heidegger at the heart of twentieth-century political theory as it moves between the Scylla and Charybdis of revolutionary passion and the search for an objective standard of reform.

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[16] See Kolakowski's remarks on Lukacs's search for an alternative to the "evolutionist" and "positivist" Marxism of the Second International (1981, pp. 253-256, 260).

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Communications

Comment on Przeworski and Wallerstein (Vol. 76, June 1982, pp. 215-238)

Adam Przeworski and Michael Wallerstein aim to provide a simple explanation of the falsification of Marx's prophecy about irreconcilable class struggle. In pursuing this objective, the authors relativize the Marxian zero-sum game assumption of class conflict, introducing a long-term nonconstant sum component in the game between capitalists and workers. Thus, they accept the Marxian assumption that the total output of the economy is divided into profits and wages, which means that each one of them is a decreasing function of the other. On the other hand, they consider what they call the "underlying logic of compromise," that wages increase as a percentage of current profits. This assumption introduces common interests between workers and capitalists concerning the survival of capitalism.

Subsequently, workers seek to maximize their wages W^*, and capitalists their consumption C^*, by manipulating their weapons: worker militancy r and the ratio of savings s, respectively, subject to the constraint that the opponent will choose the best strategy. This maximization is done over a time horizon h, with time discount rates a and b for workers and capitalists respectively. Specifically, the capitalists choose each time (for each r) their best s (s^* = the one that maximizes C^*) and in their turn, the workers choose from these pairs of r and s^* the one that maximizes W^*: (r^{**}, s^*). If r^{**} and s^* take the extreme permitted values, then there is no possible class compromise. Przeworski and Wallerstein use the term "workers dominance" for this procedure, and subsequently I focus on this case, although the arguments (under a weaker form) hold also for the dual case of "capitalist dominance."

The graphic representation of the function $W^*(r)$ is given in Figure 1. (Compare p. 229.)

This figure shows that the value of r that maximizes W^* is in the extreme right of the diagram. In the original article, however, the curve $W^*(r)$ is shown as dashed beyond the local minimum. Przeworski and Wallerstein make some additional assumptions to justify this choice (p. 227). They involve cost of militancy, political reasons, and history, and they summarize:

> When workers are highly uncertain and capitalists relatively certain, a compromise *may* [emphasis added] be established at a point at which workers are kept from increasing their militancy by capitalists' threat of disinvestment, whereas capitalists' optimal rate of investment is positive. (p. 230)

The problem with this model arises from its sensitivity and instability. As we can verify from Figure 1, $W^*(r)$ presents a local maximum (at r^{**}), and a local minimum (at $0.25 - b$). However, beyond a certain level of militancy r_{lim}, $W^*(r^{**})$ becomes a suboptimal solution. The smaller the difference $r_{lim} - r^{**}$, the more implausible becomes the authors' claim that compromise may be the dominant solution for workers. Therefore, $r_{lim} - r^{**}$ is an indicator of the fragility of the compromise solution.

Table 1 replicates Table 1 (p. 231) of the original article. We have added two columns containing the values of r_{lim}, and $r_{lim} - r^{**}$, respectively. Differences in values result from technical reasons.[1]

For $a < 0.16$ there is no local maximum of the curve, which means that under these assumptions compromise solutions are not possible.

Table 1 indicates that the "underlying logic of compromise" is not enough to assure class compromise, especially when workers are sure about the future (low a). Indeed as the future discount rate a decreases, $r_{lim} - r^{**}$ decreases dramatically. However, the results of the model seem to imply that class compromise *might* occur when workers are highly uncertain about their future conditions. There is one important problem with this conclusion. If a class is uncertain about the future, then it is not likely to maximize its objective function over a long time horizon. In algebraic terms, the future discounted wages decrease steeply, so that the workers receive in a very small number of years almost the same amount that they would receive in an infinitely long future. It seems then that under these conditions there is no point in pursuing the maximization. Table 2 illustrates this property. (The values of a, r^{**}, s^*, and W^*_{30} are taken from Table 1 of the original article (p. 231).)

The table shows that in these cases the dominant class has exceeded 95% of the target value in 12 years. If we combine this remark with Przewor-

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I would like to thank J. Sprague, B. Salert, J. Alt, and K. Shepsle for their useful comments.

[1] We maintain the condition $1/[(1+a)(1+b)] = 0.7$, and we tested a range of r [0.0, 0.35] and s [0.21, 0.99] with a 0.005 increment.

Figure 1.

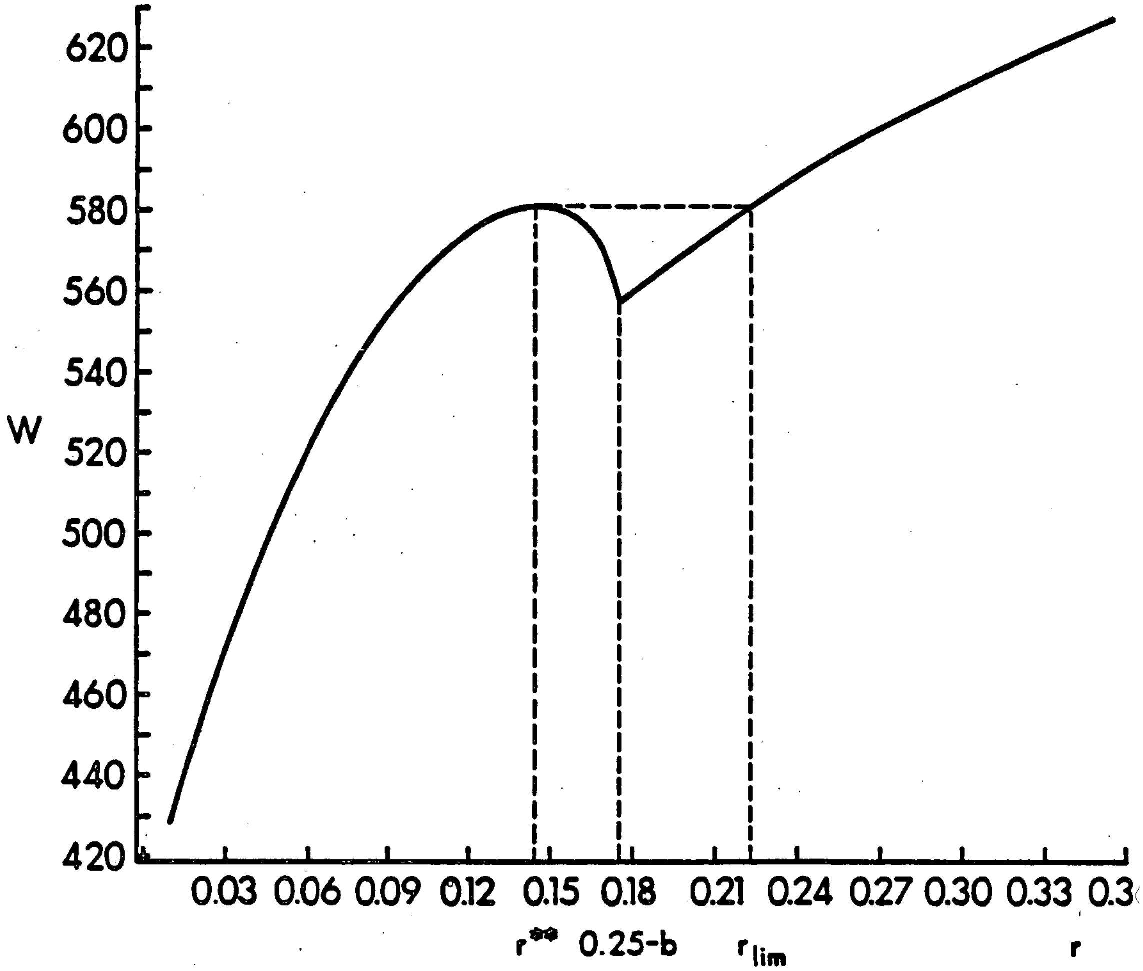

Table 1.

$h = 30$ $1/[(1+a)(1+b)] = 0.7$

a	r^{**}	W^*	r_{lim}	$r_{lim} - r^{**}$
0.421	0.210	488.8	0.305	0.095
0.331	0.150	580.6	0.225	0.075
0.231	0.065	732.9	0.110	0.045
0.161	No local maximum			

Table 2.

a	r^{**}	s^*	W^*_{30}	W^*_{12}	% of W^* attained after 12 years
0.43	0.215	0.655	483	470.6	97.4
0.35	0.16	0.615	557	532.2	95.5

Table 3.

$h = 50$, $rho = 80\%$, $1/(1+a)(1+b) = 0.7$

a	r^{**}	s^*	W^*	C^*	h_W	h_C	r_{lim}	$r_{lim}-r^{**}$
0.421	0.23	0	97.5	384.5	5	26	0.43	0.20
0.331	0.16	0.73	487.9	416.4	7	26	0.41	0.25
0.231	0.06	0.78	684.2	541.2	14	34	0.29	0.23
0.161	0.01	0.62	744.8	426.4	17	20	0.05	0.04

ski's and Wallerstein's statement that for $h < 12$ "there is no compromise solution," we come to the conclusion that also under these conditions class compromise is extremely unstable. Intuitively, under high uncertainty about the future, the Keynesian aphorism about long-term conditions and the Marxian expectations become jointly relevant.

To sum up: The original idea of finding some relatively simple structural conditions that would explain the failure of Marx's prophecy is very interesting. But the "brake" proposed for irreconcilable class struggle is not enough because, in the case of low future discount rates, the local maxima reported in the article have a very limited range of dominance and, in the case of high future discount rates, maximization over a long time period is unlikely to occur.

In light of these remarks, there is a technical modification that increases the stability of the model. One can assume that both classes will maximize over a long *nominal* time horizon h, but will stop the process in shorter *actual* time horizons when they reach a certain percentage *rho* of their long-term target. Under this assumption, both workers and capitalists will not choose r^{**} and s^* that maximize W^* and C^* over a long time horizon, but the ones that will give them *rho*% of these targets in a smaller time interval.

Let us call h_w and h_c these actual time horizons for workers and capitalists respectively. For the original model $rho = 100\%$ and $h_w = h_c = h$. However, as *rho* decreases, an additional trade-off mechanism between long-term considerations and militancy arises. The actual time horizon becomes endogenous to the model (function of $a, b, r, s,$ and *rho*). Since the number of terms in both W^* and C^* is a function of this endogenous actual time horizon, each actor may be able to get more by increasing this actual time horizon than by increasing his militancy. Table 3 demonstrates the results of incorporating these ideas for $h = 50$ and $rho = 80\%$.

Table 3 demonstrates that the more uncertain workers are about the feature, the more this actual horizon decreases. By comparing the column $r_{lim} - r^{**}$ of Table 3 with the corresponding column in Table 1, we can understand the stabilizing effect of the new assumption. Although the initial assumption was about a 50-year nominal time horizon, the actual time horizon of each class exceeds 30 years only once. Indeed, the additional "brake" operates by increasing the nominal time horizon and consequently "transposing" to more limited actual time horizons equilibria that would otherwise (in the unmodified model) need a nominal time horizon of 50 years to take place. But the stabilizing action of the *rho* threshold is not limited to this effect. An additional trade-off mechanism between actual time horizons and militancy has been installed. Even for a nominal time horizon $h = 30$ (actual horizons h_w and h_c less than 20 years) the stability of the model increases substantially.

In summary, an additional mechanism was introduced that increased the plausibility of compromise solutions between workers and capitalists. This mechanism has a double effect. First, it permits more weight to the compromise conditions through the increases in the nominal time horizon whereas the actual time horizon remains limited. Second, it installs for both classes a trade-off between actual time horizons and militancy that has an autonomous effect on the intensity of class struggle. A comparison of Tables 1 and 3 shows that $r_{lim} - r^{**}$, which is the indicator of the stability of the model, has at least been doubled. However, it is incorrect to consider the problem resolved. On the contrary, the original challenge remains, and a more credible model must be advanced to explain the failure of the Marxian prophecy.

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Reply

Unfortunately, the preceding comment on "The Structure of Class Conflict in Democratic Capitalist Societies" is based on an erroneous understanding of our original argument and contributes little of substance. Because the blame for some of the confusion may be ours, we are happy to clarify.

Professor Tsebelis states that in our model: " 'the underlying logic of compromise' is not enough to assure class compromise, especially when workers are sure about the future (low a)." This is simply incorrect. In our model, a compromise solution, that is, a solution in which capitalists invest at a positive rate and workers refrain from claiming the entire product, always exists when either class bears relatively low risk. The only case with no compromise solution is the case in which the risk facing both classes is relatively high (specifically, when the rates of discount, a and b, both exceed the productivity of capital, $1/c$).

How did Tsebelis draw the opposite conclusion? It appears by misreading our Table 1, reproduced as Table 1 in the "Comment," in two ways. To explain, we must review the nature of our solutions. They are Stackelberg solutions in which one class leads and the other class follows. The follower responds with its best reply, that is, with its optimal strategy given the actions taken by the leader. Since the follower's best reply is predictable, it is anticipated by the leader when deciding which action to take. The actor that leads is often called the dominant player. It is important to note that there is no general advantage to being the dominant player. In our model, each class is better off if the other is forced to lead.

Our results, then, were the following: There exists a compromise solution with workers as the dominant player when capitalists' risk is low (when $b < 1/c$) and another solution with capitalists as the dominant player when workers' risk is low (when $a < 1/c$). (There does not exist a compromise Nash solution under any circumstances.) Professor Tsebelis's error was to conclude that the solution with workers dominant ceases to exist in Table 1 because the workers' rate of discount a falls below 0.16. In fact, the existence of this solution depends in no way upon workers' discount rate. What he missed is that with our specified relationship between a and b, $1/[(1+a)(1+b)] = 0.7$, $b > 1/c$ when $a < 0.16$, and it is the increase in the capitalists' rate of discount to a level at which capitalists refuse to invest regardless of workers' actions that destroys the compromise with workers dominant.[1]

Tsebelis's second misunderstanding stems from failing to consider the entire Table 1, including the part not replicated in his "Comment." What the other half of Table 1 shows is that when $a < 1/c$ the solution with capitalists as the dominant player exists and that this solution is a much better deal for workers. Finally, we must emphasize that the particular relationship assumed in Table 1 between a and b was chosen for illustrative purposes only. In general, there is no reason why reductions in the risk facing workers must always be accompanied by increases in the risk for capitalists.

There is another, nontechnical, and more important misunderstanding in the "Comment." This concerns the comparisons of workers' welfare at the local maximum under capitalism, $W^*(r^{**})$ in Figure 1, with workers' welfare when they are highly militant (when r is large). If the line we drew on the right side of $0.25 - b$ in Figure 1 was a dashed one, it was for good reasons, reasons that were stated on page 227 and discussed in the section entitled "Beyond Capitalist Democracy" (pp. 233-235). To keep the discussion as simple as possible, let us assume that $a > 1/c$ and $b < 1/c$ so that the solution illustrated in Figure 1 is the only possible compromise. If workers increase r beyond r^{**}, capitalists respond by reducing investment sharply, leaving workers worse off. If workers continue to increase r until they reach $r = 1/c - b$, capitalists respond by disinvesting at the maximum rate possible. Beyond this point workers have nothing to lose and will only gain if they grab a greater and greater share of the profits, provided the laws governing the normal class relations of democratic capitalist societies, in particular equation (7) (p. 217) of our text, continue to hold. But it is implausible that such laws would continue to hold in a crisis as capitalists disinvest, and workers become increasingly militant. To the right of $0.25 - b$ lies either a socialist transformation or an authoritarian capitalist restoration. In Figure 1, the function $W^*(r)$ is no longer defined for r in this region.

To illustrate the point, we have redrawn Figure 1 as we should have drawn it in the article to avoid confusion. $W^*(r^{**})$ is the best workers can do under capitalism, and S^* is the expected value to workers of a revolutionary strategy as defined on page 234 of our text. (S^* includes the risk of an attempted socialist transformation being aborted by a counterrevolution.) If $W^*(r^{**}) > S^*$, the local maximum under capitalism is, in fact, a global

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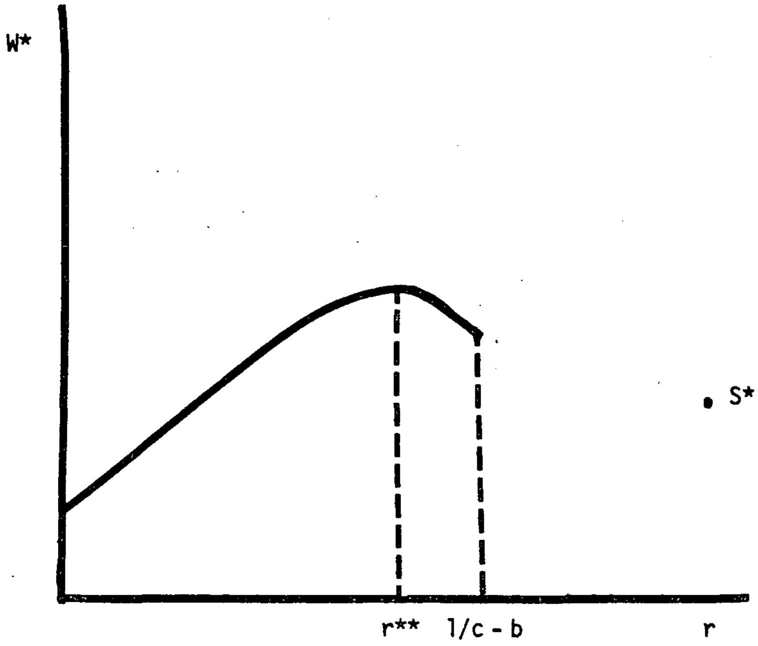
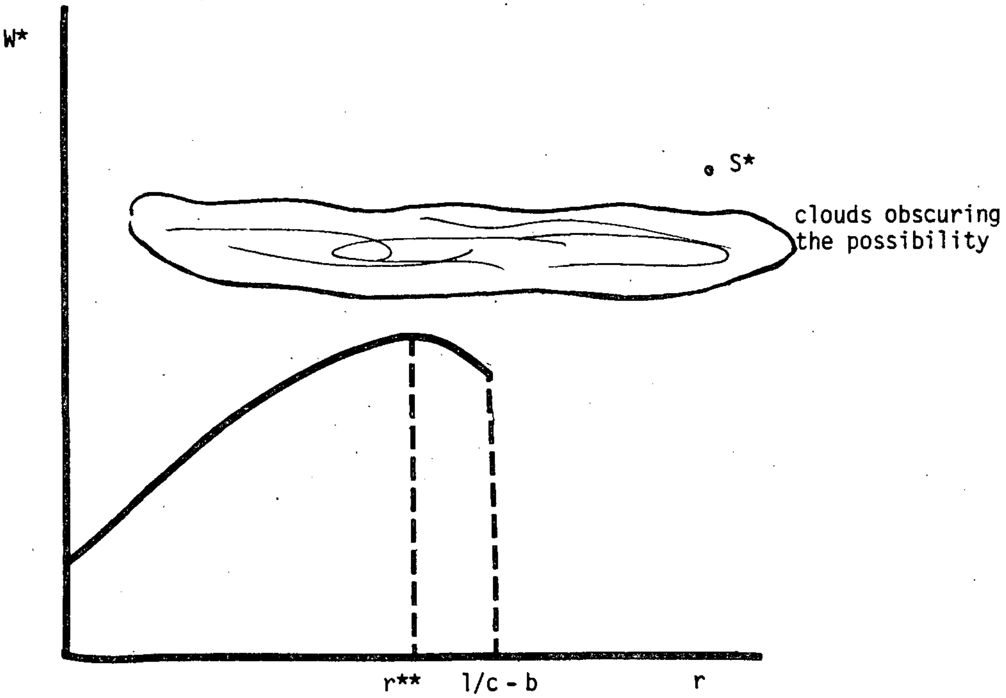
[1] There is a technical confusion in our text. With $1/[(1+a)(1+b)] = 0.7$, $b > 0.23$ when $a < 0.16$, whereas we assume throughout that $1/c = 0.25$. This discrepancy is due to the difference between our analytic results, which assume an infinite horizon, and our simulated results in which the horizon was taken to be 30. Formally and correctly, the proper threshold, call it T, should have been written $T = 1/c - \epsilon(h)$, where $\epsilon(h) > 0$, $\epsilon'(h) < 0$, and $\epsilon(h) \rightarrow 0$ as $h \rightarrow \infty$.

Figure 1. If the Expected Value of Socialism Is Lower than the Capitalist Maximum



If the Expected Value of Socialism Is Higher than the Capitalist Maximum



maximum. It is the best workers can do under the circumstances in any system. Because we do not know how to assess the value of S^*, we left it as a possibility, perhaps a temptation, that workers who have maximized their capitalist fate by choosing r^{**} might yet opt for a revolutionary strategy. The distance $r^{**} - r_{lim}$ is of no significance whatsoever. The choice facing workers around r^{**} is not whether to move into the region of high r. Once they go past r^{**} they will have decided to go all the way to socialism or barbarism. The only relevant comparison is of $W^*(r^{**})$ and S^*, and who knows how to make this comparison.

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Comment on Kramer (Vol. 77, March 1983, pp. 92-111)

Gerald Kramer has played a pivotal role in the study of economic voting. His 1971 article was a watershed; it directed research on economic voting away from using "rather simple statistical methods. . ." toward methodological sophistication (1971, p. 133). With aggregate-level data, Kramer was able to identify a relationship between economic conditions and vote. Some subsequent studies using individual-level data, however, "have been unable to detect any comparable relationship between individual voting behavior and personal economic circumstances" (Kramer, 1983, p. 92). In "The Ecological Fallacy Revisited" Kramer attributes the apparent incongruity between these two methods of studying economic voting to a statistical artifact in the individual-level studies. He concludes that the aggregate-level studies provide better estimates of economic voting than the individual-level research. This comment critically examines Kramer's "Ecological Fallacy" article, and particularly its basic assumptions.

Kramer bases his study on three assumptions which are untenable and which point to conclusions concerning aggregate research that are similarly not supportable. First, Kramer is interested in how "*real* economic outcomes affect *actual* voting decisions and not in economic rhetoric or perceptual imagery" (1983, p. 95). Kramer's measure of real economic outcomes is change in per capita income. Kramer's second assumption is that economic voting is correctly studied by estimating the effect of government-induced economic changes on voting behavior. Non-government-induced changes bias the estimation of economic voting. Change in one's income, Kramer suggests, results from both government-induced and "politically irrelevant," "extraneous" factors. Large extraneous effects, the OPEC embargo for example, have national implications and bias both the aggregate and individual studies. However, in the individual studies bias occurs because the local extraneous effects cannot be separated from those that are government-induced. Extraneous factors bias both the aggregate and individual estimates of economic voting with the greater bias, a consequence of local economic changes, affecting the individual research.

Third, Kramer assumes that the "effect of interest" (1983, p. 95) is not individual elections, but rather the pattern of elections over time. Individual elections may present counterintuitive or "spurious" results that would not be evident in the aggregate studies.

Kramer's assumptions permit a mathematical demonstration of how the less biased aggregate estimates of economic voting are superior to the individual-level approach. While all of this works well within the economic world that Kramer creates, his assumptions are unrealistic in the political world. Assumptions that more correctly reflect political reality would result in different conclusions.

Kramer's first assumption is that the economic vote is based on a change in per-capita income. Unfortunately, he does not describe the cognitive process by which voters calculate their net income change. Except in the most general terms, of course, voters are not able to determine the net change in their income. A voter's economic situation is largely determined by subjective interpretations of many individual and societal factors. Per-capita income is merely an imprecise measure of only one aspect of personal financial situation.

Individual-level studies use survey data that assess the economic status of respondents by asking whether their financial situation has gotten better, worse, or remained the same. The survey data, which measure subjective interpretations of objective factors, better represent voters' financial situation than income change which taps only the objective conditions. There is empirical evidence to support the superiority of survey data as a measure of economic condition. Inflation, for example, is an important factor affecting income levels. Yet Hibbs (1979, p. 712) finds that "less tangible subjective and psychological factors are more important than objective costs in explaining widespread aversion to inflation." Fiorina (1981, p. 29) discovered that family financial data (SRC survey) were a better predictor of presidential and congressional elections than income change. Logically and empirically the survey data better measure personal financial condition than income change, and therefore more accurately estimate economic voting.

Kramer's second assumption is that only government-induced impacts on the economy should be considered when estimating economic voting. It is unlikely, especially in our complex economy, that voters are able to distinguish accurately between effects that are or are not government induced. The closing of a steel factory could be the result of outmoded production processes, but unemployed workers may blame the administration's high interest rates that limit auto purchases. It is not the source of financial discomfort that predicts economic voting, but rather the attribution of blame to the government. Feldman (1982, p. 463) demonstrates that self-interested political behavior is most likely to result when societal conditions or political actors are seen as responsible for economic grievances. It is the individual-level data that can best unravel the complex relationship between personal financial condition and political actions. Again, both logically and empirically, the individual-level data are more likely to provide better estimates of economic voting.

Kramer's third assumption excludes the analysis of individual elections which may, he suggests, exhibit spurious results. Kramer implies that there is a constant linear relationship between economic conditions and vote over all elections; this may be wrong. Economic voting may not have been evident in the prosperous years of 1964 and 1966 even among the disadvantaged who could not blame the in-party for their misfortune when the economy was booming (Alford, 1982, p. 17). Similarly, Bloom and Price (1975, p. 1241) found economic voting in a downturn "while economic upturns yield no corresponding benefits." Since, as demonstrated previously, Kramer's model cannot adequately measure personal financial situation nor detect "bias," he cannot determine if a particular election result is spurious. The counterintuitive findings evident in specific elections are not likely the result of methodological artifacts, but rather reflect real political events.

Assumptions that differ from Kramer's, and more accurately mirror political reality, would lead to the conclusion that individual-level studies can provide accurate estimates of economic voting. This is not to argue that we should cease in our efforts to improve individual-level research; we need to increase the precision of our estimates of economic voting. Nor is this intended to be a general attack on aggregate research of economic voting. Aggregate research, especially studies based on realistic assumptions about the political world, can yield important information. We should discard the notion that a competition exists between aggregate or individual-level studies; each approach contributes in different ways to our understanding of economic voting. Aggregate studies permit us to see patterns and change in economic voting, whereas individual-level research allows a more complete understanding of political factors and vote performance in each election. A fruitful task would be to examine accurately the strengths and shortcomings of each approach and then to assess their ultimate contribution to our knowledge of economic voting.

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Reply

Michael Scicchitano feels that my conclusions rest on three assumptions that are "untenable" and "unrealistic in the political world."

Taking his points in reverse order, the third seems based on a misunderstanding of the logic of the argument. I used the assumption that the same relationship operates in all elections for two purposes: First, to show that, even so, individual and aggregate-level analyses would still yield different results, and second, that the latter will often yield a reasonably good estimate of the true relationship. The assumption played no role whatever in the demonstration (pp. 100-102 and 109) that an individual-level cross-sectional analysis will fail to detect the true (individual-level) relationship. If, as Scicchitano suggests, this true relationship varies from election to election, individual-level analyses will simply fail to measure *any* of them correctly. Of course, an aggregate-level analysis would also fail under these conditions (unless the differences were of the Bloom-Price type or exhibited some other regularity, in which case an aggregate-level analysis of the proper kind might again succeed), but in no sense does this resurrect or justify individual-level cross-sectional analysis as a viable alternative.

With respect to the second assumption, Scicchitano argues that it is "unlikely . . . in our complex economy, that voters are able to distinguish accurately between" government-induced and other effects. I agree. But he evidently feels that, since voters can't make these distinctions *accurately,* they don't make them at all. Thus, Scicchitano would presumably maintain, for example, that the strain on a family's financial well-being that would arise from sending a child to an expensive private college or from having their uninsured home burn down is just as relevant politically as a tax increase of equal magnitude. This seems to me an untenable assumption for the political world I perceive. I think most voters would react quite differently to these events, and that in general they can and do recognize, at least roughly, the difference between government-induced and other, nongovernmental effects.

Scicchitano's remaining point concerns the distinction between self-reported subjective perceptions versus objective measures of personal economic well-being; he feels the former "more accurately estimate economic voting." Perhaps this is simply a linguistic or terminological problem. For example, consider a scenario in which the economy hummed along evenly under a single incumbent over a series of elections with absolutely no measurable differences in any aspect of any individual's economic condition from one election to the next. Suppose, at the same time, there was considerable variation in individual voting behavior over the period and that it were found that voters (perhaps having been conditioned by Reagan's 1980 "ask youself. . ." slogan) invariably reported being better or worse off financially according to whether they were going to vote for or against the incumbent. Scicchitano would presumably interpret this as evidence for "economic voting." I, on the other hand, would say this shows nothing at all about how economic circumstances affect voting behavior (it couldn't, because there was no variation in any of the economic variables), although it may reveal something interesting about the way in which voters rationalize their choices or form perceptions. I suppose Scicchitano is free to define "economic voting" as he chooses, but I must say it seems to me greatly preferable to recognize and preserve the distinction between subjective psychological and objective economic variables, in the interests of both linguistic clarity and scientific understanding.

I agree with Scicchitano on the desirability of more detailed election-specific (or group-specific) studies. However, I'm afraid I remain unpersuaded that cross-sectionally oriented individual-level studies (especially those based on subjective measures) can contribute meaningfully to that goal.

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Reply to Hill and Cassel (Vol. 77, December 1983, pp. 1011-1012)

Our article (Abramson & Aldrich, 1982) demonstrated that weakening party loyalties and eroding beliefs about government responsiveness played a major role in accounting for the decline of turnout from 1960 to 1980. Hill and Cassel argue that our research is misleading. Their central claim is that "the goal of identifying a simple and elegant model of declining participation has been perverted by statistical and *ceteris paribus* gymnastics which seek to maximize the explanatory power of this simple model at the expense of completeness and necessary complexity" (Hill & Cassel, p. 1011). They argue that we left out important social and demographic variables, that our model is "fundamentally misspecified" and that, therefore, we cannot claim "to have solved Brody's (1978) much discussed 'puzzle' of declining electoral participation" (p. 1011). As examples of misspecification, they maintain we should have included residential mobility and education in our estimates.

Because we do not include any social or demographic variables in our analysis, we obviously exclude some that are relevant to understanding changes in postwar turnout, along with many that are not. We discuss at some length our research strategy of focusing on two attitudinal variables (Abramson & Aldrich, 1982, p. 504; note 20, pp. 511-512). We emphasize that our "analysis has not attempted a comprehensive explanation of the changes in attitudes themselves" (p. 519), a step necessary for any full theoretical account. We discuss other attitudinal variables, especially declining concern about electoral outcomes, but do not directly incorporate them in our estimates. Most important, we acknowledge that our analyses "do not discriminate between alternative theoretical views of voting behavior" (p. 519).

We obviously never claimed to have developed a fully specified model of the decline of turnout, and nowhere did we claim to have "solved" the puzzle of declining electoral participation. Indeed, our basic conclusion that "the combined effect of the decline in partisan strength and the decline in beliefs about government responsiveness appears to account for between two-thirds and seven-tenths of the decline in presidential turnout" (p. 519) makes it clear that the puzzle remains unsolved. We do claim that the two variables we analyzed go a long way toward explaining why turnout declined. But given the ranges of the values we regularly report, it should be apparent that we consider our numerical estimates as indications of the approximate degree of explanation.

Although we do not provide unbiased point estimates, we have seen no evidence that our equations yield misleading approximations. Hill and Cassel offer no empirical evidence that the purported misspecification has any impact on actual estimates. Let us look at the two variables they suggest might have an impact—residential mobility and education.

Hill and Cassel state that "without a variable for voter mobility in the Abramson and Aldrich analysis, one does not know whether the apparent contribution of their two variables to the decline is undermined by spurious correlations" (1983, p. 1011). They assert that their earlier work demonstrated that "the increasing mobility of voters made a significant contribution to declining turnout." But this is not what they actually found. Their earlier analysis of SRC-CPS data (Cassel & Hill, 1981) showed that residential mobility was significantly related to turnout in both 1964 and 1976 (p. 187). But, according to their own estimates, increased residential mobility accounted for only 0.5% points of the 10% point decline in non-Southern turnout (pp. 189, 191). A variable with such a weak impact would not affect the contribution of party identification and feelings of political efficacy very much, even if it were highly correlated with these attitudes. In fact, it is not. Cassel and Hill (1980, Table 1A) report that in 1976 the zero-order correlation between residential mobility and partisan strength was -.09, whereas the correlation between mobility and their measure of political efficacy was .04.

Turning to level of education, Hill and Cassel (1983, p. 1011) "cannot help but wonder how much of the explanatory power of the authors' two-variable model would evaporate if the offsetting power of education were introduced into the model." Of course, including education would be necessary to fully explain variation in postwar turnout, and we have demonstrated elsewhere that increased educational levels played a major role in impeding the decline of turnout (Abramson, Aldrich, & Rohde, 1982, pp. 84-85). But given the increases in educational levels between 1960 and 1980 and the positive relationship between education and political efficacy and education and turnout, one would expect in the abstract that including education as a "control" would strengthen the impact of the decline of efficacy on the decline of turnout.

The actual effect of controls is ultimately an empirical question, and the evidence shows that controls for education do not cause the explanatory power of party identification and feelings of political efficacy to "evaporate." In his recent book, Kleppner (1982, pp. 122-130) studies declining turnout among non-Southern whites, using algebraic standardization procedures very similar to ours. Using SRC data for the 1952 through 1980 period, he estimates the impact of weakening party loyalties and feelings of political efficacy (measured as we did). Kleppner's calculations include simultaneous controls for education, income, age, and sex. Despite these controls, his results for non-Southern whites are very close to ours for the entire white electorate. We find that between a fourth and three-tenths of the decline in turnout from 1960 to 1980 can be accounted for by weakening partisan loyalties, whereas Kleppner's estimate is 28.5%. We find that just over half the decline can be attributed to eroding feelings of "external" political efficacy, whereas Kleppner's estimate is 52.6%. Kleppner's research not only supports our position that these two attitudinal variables made a substantial contribution to the decline of turnout, but also shows that their impact persists when controls for social and demographic variables are introduced.

Our goal was to demonstrate that attitudinal change made an important contribution to the decline of turnout. Including additional attitudinal variables would affect our estimates, as would including relevant social and demographic variables. Extant research suggests, however, that including residential mobility would have very little effect on our estimates, and that including level of education would not weaken them. Knowing what other variables to include in order to attain unbiased point estimation requires proper specification. Attaining such specification is basically a theoretical task, and only theory can unravel the puzzle of declining turnout. What is really important at this stage is not questioning the specific importance of particular variables, but doing the hard theoretical work needed to sort out the relationships among explanatory variables. By demonstrating the importance of two key attitudinal variables, and by clarifying some of the conceptual confusion that has characterized their study, we hope we contributed to that larger goal.

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Errata

In "Changes in the Vote Margins for Congressional Candidates: A Specification of Historical Trends" by James C. Garand and Donald A. Gross (Volume 78, pp. 17-30), Figures 2 and 3 were inadvertently transposed. The caption for Figure 2 (p. 21) should have been placed with the graph on p. 27, and the caption for Figure 3 should have appeared over the graph on p. 21. We regret the error.

"A Method of Estimating the Personal Ideology of Political Representatives" by Richard T. Carson and Joe A. Oppenheimer (Vol. 78, pp. 163-174) contained several errors. In Table 3, Senator Leahy (Vt.) should have been identified as a Democrat with an I* of .17, and Senator Stafford (Vt.) as a Republican with an I* of -.29. These errors did not affect the other data as reported. On pp. 173 and 175, Senator Leahy is cited as an example of a "Republican" given a boost as a liberal who bucked his party and who is given a high liberalism score; the references should be to Senator Weicker. Also in Table 3, Senator Reigle should have been listed as from Michigan and Senator Pryor as from Arizona.

Forthcoming Articles

The following articles have tentatively been scheduled for publication in the December 1984 issue.

B. A. Anderson and B. D. Silver, "Equality, Efficiency, and Politics in Soviet Bilingual Education Policy, 1934-1980"

M. K. Jennings and G. B. Markus, "Partisan Orientations over the Long Haul: Results from the Three-Wave Political Socialization Panel Study"

D. Mason, "Individual Participation in Collective Racial Violence: A Rational Choice Synthesis"

M. Midlarsky, "Political Stability of Two Multiparty Systems: Probabilistic Bases for the Comparison of Party Systems"

P. F. Nardulli, R. B. Flemming, and J. Eisenstein, "Unraveling the Complexities of Decision Making in Face-to-Face Groups: A Contextual Analysis of Plea Bargained Sentences"

R. G. Niemi, "The Problem of Strategic Behavior under Approval Voting"

L. M. Preston, "Freedom, Markets, and Voluntary Exchange"

L. Ragsdale, "The Politics of Presidential Speechmaking, 1949-1980"

D. R. Sabia, "Political Education and the History of Political Thought"

J. A. Segal, "Predicting Supreme Court Cases Probabilistically: The Search and Seizure Cases, 1962-1981"

R. M. Siverson and M. R. Tennefoss, "Power, Alliance, and the Escalation of International Conflict 1815-1965"

S. S. Ulmer, "The Supreme Court's Certiorari Decisions: 'Conflict' as a Predictive Variable"

BOOK REVIEWS

American Government and Politics

Financing the 1980 Election. By Herbert E. Alexander. (Lexington, Mass.: D.C. Heath, 1983. Pp. xii + 524. $34.95.)

Campaign funding reforms enacted in the 1970s have provided an informational treasure house to researchers interested in documenting the campaign process. Herbert Alexander's latest volume, his sixth study on the topic, reaffirms his status as a doyen in the field. Concomitantly, the book also provides some testimony to the argument that unless students in this area attempt to properly integrate data presentation with some theoretical perspective, what emerges is an overwhelmingly confusing and mind-numbing array of disconnected facts.

Works like this one, which are efforts to incorporate voluminous reference material in a textbook format, are difficult to organize cohesively, an observation even Alexander admits to in his preface (p. xi). The material is organized according to the chronology of the campaign process, which creates some repetition of findings and raises questions about the order of topics examined in some of the chapters. Certainly, summaries at the end of each chapter would have been useful. However, as a reference book much valuable information is introduced and discussed. In fact, comprehensive coverage of the topic is the book's main strength. Ample and well-presented documentation accompanies the chapters concerning the amount and pattern of campaign expenditures in all national contests during the 1980 election year cycle. This discussion is followed by chapters dealing sequentially with the prenomination, convention, and general election periods containing extensive accounts of campaign spending during each phase. Information about political action committees and individual contributions in 1980 is then exhaustively explored. The postelection funding concerns and activities of both major parties' national committees receives the bulk of attention in the "Aftermath" chapter. The chapter includes a brief look at the National Conservative Political Action Committee's preparations for the 1982 congressional elections, which underscores the perception that unlike the electoral process, the campaign finance process seems to have no identifiable beginning and end points. A much too perfunctory description of suggested future reforms of the campaign finance rules anchors the entire study. Throughout these largely narrative chapters, a most interesting recurring discussion concerns how the courts and the Federal Election Commission have litigated the diverse and complicated questions that emerged as a result of the early reform laws. Any academic course that addresses the issue of campaign finance would be incomplete if this critical dimension were ignored or excluded.

The study's most disappointing feature reflects the absence of an underlying framework to help link the enormous amount of information with the question of what it all means. After the major themes are summarized in the introductory chapter, there is no analytical compass provided in the remaining sections which can guide the reader to address this substantively critical question. For example, if, as Alexander argues, the reform laws alone should not be faulted for promoting the diminishing role political parties play in campaign activities, on what basis can he then conclude that the reforms may actually be responsible for rejuvenating the parties? Moreover, the reader is left wondering why, of all the prospective reforms outlined in the study, Alexander prefers those aimed at providing the political parties greater fund-raising and spending discretion as most beneficial. An interpretive concluding chapter containing an in-depth examination of the general findings presented in the Introduction would have been a helpful addition, even at the cost of condensing some of the data-based chapters.

The book's self-declared intention is to "illuminate the role money plays" (p. 6) in America's electoral process. A great deal of light is shed, to continue with the metaphor, on a range of finance-related factors involved in the selection of national-level public officials. However, with such vast and varied details in view, the images presented in an unfocused light become somewhat blurred after a while, even to the most diligent reader.

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Working Class Hero: A New Strategy for Labor. By Stanley Aronowitz. (New York: Pilgrim Press, 1983. Pp. xvii + 227. $18.95.)

Working Class Hero is ambitious, provocative, and exasperating. Aronowitz examines the crises that afflict the American labor movement today and the reasons that have led to its industrial and political decline. He argues that the labor movement needs a new strategy that is appropriate to the conditions it faces: deindustrialization, the feminization of the work force, the steady rationalization of work, and the growth of technical and intellectual labor. The final chapter of *Working Class Hero* outlines what such a strategy might entail. Aronowitz argues that the labor movement should form a new political bloc with social movements that raise such "postmaterialist" issues as feminism, black liberation, and self-management. These issues speak to the needs of women, blacks, and the "technical intelligentsia," workers who are located in the new regions of capitalist growth. According to Aronowitz, the revival of the labor movement depends upon its ability to go beyond economism and raise postmaterialist demands that appeal to workers in sunrise industries.

Despite the considerable knowledge and originality that Aronowitz brings to his discussion of these issues, the book irritates. The argument is discursive, generalizations abound, and although many issues come under review, few are treated with the depth that could substantiate them. So many topics compete for space in Aronowitz's argument—everything from the effect of video display terminals on workers to the New York City fiscal crises—that less would have been more in this case.

The book is divided into three parts. The first third argues that Samuel Gompers was responsible for the development of a distinctive labor philosophy called corporativism. Aronowitz contends that this ideology, in which unions renounce a class model of opposition to capital except with regard to workplace issues, still guides the theory and practice of American trade unionism. Yet any discussion of Gompers's legacy is a tricky matter, because he left several. The same man who was a class-conscious syndicalist was also a member of the National Civic Federation. "Gompersism" was more contradictory than Aronowitz's discussion would indicate.

The second and third parts of the book are the most innovative and provocative. Here, Aronowitz argues that late capitalism has marginalized the old industrial working class and created a new working class made up of women, minorities, and the "technical intelligentsia." These groups find the old business unionism insufficient and engage in a more fundamental cultural critique of American capitalism with their demands of feminism, racial justice, and the dignity of labor. But Aronowitz overstates his case. First, he tends to view the demands of this new working class uncritically. He sees the demand of intellectual workers for professionalism as a means whereby these workers can prevent their work from being rationalized and degraded by management. But professionalism also preserves artificial hierarchies within the work force and is a way in which knowledge workers, the "technical intelligentsia," can protect their privileges against other workers. Second, empirical work by Duncan Gallie and John R. Low-Beer has shown that the demands of this new working class do not differ greatly from the old. Moreover, there is a surprising "workplace determinism" applied to the politics of knowledge workers in which they are radicalized by the rationalization of their work that Aronowitz rejects explicitly elsewhere in the book. Lastly, Aronowitz minimizes how divisive such issues as feminism or consumerism are among both the new and old working class. Demands such as redistribution offer a greater chance for unity among working-class factions and are more likely than postmaterialist demands to take on a new relevance in the zero-sum economy of the future.

Economic decline has reduced living standards throughout the working class. It is hard to imagine clerical workers on the picket line for on-site day care in the midst of the new austerity. The demand for "more" is likely to take on a new urgency and a redistributive cast under these conditions. The revival of the labor movement may depend less on raising new postmaterialist demands that are divisive than on its ability to demand more for all working-class factions when the pie is shrinking.

ALAN DRAPER

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Justice without Law? By Jerold S. Auerbach. (New York: Oxford University Press, 1983. Pp. xiii + 182. $16.95.)

There have been two major challenges to the equanimity of the American legal profession in the past 15 years. Jerold Auerbach's classic *Unequal Justice* (Oxford University Press, 1976) dealt with the first, when young, idealistic lawyers, law professors, and law students demanded a more socially responsible profession during the late 1960s and early 1970s.

The second challenge, one which promotes "non-adversarial" alternatives to the courts, is underway presently. *Justice without Law?* is Auerbach's analysis. It is generally an excellent work, but it has a fatal flaw.

Its excellence is as a historical monograph. Auerbach masterfully chronicles how the American legal profession has consistently routed the forces of nonconfrontational dispute resolution throughout American history. The details are illuminating, the theme clear, the writing skillful.

According to Auerbach, a changing America—in terms of its values and mores—made the difference. The original colonists, with their strong sense of cooperation and community and their commitment to Christian values, favored non-adversarial methods of resolving conflict. Some colonies even outlawed lawyers.

But growing individualism, increasing materialism, rampaging commercialization, rampant secularization, and the triumph of the corporate way of life combined to elevate the modern litigous system to its present predominant position.

Yet, as Auerbach reports, there have been numerous experiments in "non-legal dispute resolution" throughout American history. Some were native (certain Indian tribes). Some were transplanted (most notably from the Chinese and Jewish cultures). Some were outgrowths of communal or fundamental religious beliefs, in "backwoods utopias." At best, remnants remain.

In fact, only one type of "informal justice" has thrived throughout American history: commercial arbitration and mediation. Auerbach believes this is because industrial and business values are consistent with the prevailing contemporary American ethos, whereas various religious, ethical, and cultural values are not. Thus, although labor arbitration and mediation have great track records, community and other types of mediation have not. Therefore, Auerbach predicts that the present mediation movement will also fail.

Auerbach's fatal flaw, then, results from his leaving the path of historical accounting and interpretation and worshipping at the altar of historical determinism. Although he is correct that America demonstrates a steadily increasing appetite for litigiousness, this is not proof the trend will continue, gobbling up mediation as it grows.

Moreover, the movement towards alternative modes of dispute resolution in America today is more complex than Auerbach realizes. It is not a yearning for "community" or a nostalgia for America Lost. Ergo, it is not something the legal profession can completely co-opt, even though the courts and bar associations are moving in that direction.

The truth is that the contemporary American mediation movement is a new species. Instead of being based on religious and cultural values, it is an integral part of a new and larger transformational surge coursing throughout American society. It is part of a network of changes in social, economic, and political values conceptualized by terms such as the "third wave," "new age politics," "the aquarian conspiracy," and "megatrends." The mediation movement is only one manifestation of how people in the postindustrial, information age are trying to cope better with one another in all walks of life including families, personal relationships, the workplace, and the marketplace. As such, it can be nibbled by the legal profession, but not swallowed whole.

To the contrary, it is more likely that the legal system will feel compelled to adapt to the mediation movement, much like the medical industry has had to respond to the personal fitness revolution that has recently been sweeping America.

So, where Auerbach believes that destiny has already passed harsh judgment on the fate of the modern American mediation movement, I believe the jury is still out. And that is why I believe *Justice without Law?* proves that Jerold Auerbach is a much better legal historian than he is a social theorist.

TED BECKER

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The Burger Court: The Counter Revolution That Wasn't. Edited by Vincent Blasi. (New Haven, Conn.: Yale University Press, 1983. Pp. xiv + 326. $25.00.)

This book of original essays by some of the nation's leading constitutional scholars offers a fine description of the doctrinal developments by the Burger Court and how they differ from those of the Warren Court. The book is far less successful in explaining why the Burger Court made its choices and how the Burger Court era differs from that of the Warren Court.

The scholars are agreed that the Burger Court can be called activist. They explore how the Burger Court has recast the doctrine so that there are important new directions in the law, not prevalent in the Warren Court. Yale Kamisar emphasizes that the Burger Court usually has gone to, and turned back from, the brink of overturning landmark Warren Court decisions which protected defendant rights. Robert Burt shows how in the area of family law the Burger Court has moved from the Warren Court faith in the rationality of law and judicial choice in determining parental, children, and family rights to a faith not in all-parental authority, but rather in such authority when it is exercised in a traditional authoritarian manner. Other scholars emphasize that compared to the Warren Court, the Burger Court has less rigorously protected the freedom of the press (Thomas Emerson), no longer believes in the "maximum protection" of the freedom of expression except where speech and private property interests coalesce as in the area of commercial speech (Norman Dorson and Joel Gora), and has chosen in antitrust law to use economic efficiency standards rather than the Warren Court's reliance on market share which protected small business (R. S. Markovits). Paul Brest and Ruth Bader Ginsberg argue that in the areas of race and sex discrimination the Burger Court has broken new ground in its early years in protecting against race and gender discrimination but has moderated the impact of these expanded rights principles in later years. Most significantly, Blasi and Robert Bennett stress that the Burger Court has stopped the Warren Court movement toward making wealth classifications in the law subject to strict scrutiny under the equal protection clause.

In two excellent summary essays, Vincent Blasi and Martin Shapiro emphasize that the Burger Court has been far more interested than the Warren Court in redefining the allocation of decision-making power and in rejecting a pure equality principal.

The reason for the title seems to be that it suggests a point of view expressed in the Foreword and the summary essays by Blasi and Shapiro: that unlike the Warren Court, the Burger Court, especially its majoritarian centrist bloc justices, is less principled in its choices, less coherent in its values about equality, and most significantly, less ideological. Vincent Blasi praises the centrist Justices on the Burger Court (Blackmun, Powell, White, Stewart, and Stevens) for their disinterested, intelligent, open-minded, compassionate, dedicated pragmatism that serves as a counter-force to the excesses and irrationalities of contemporary government. Justices at the "extremes" (Brennan, Rehnquist, Marshall, and Burger) are criticized for being ideological (read rigid) and for not serving well in the traditional role of those on the extremes, innovation in principles. In the Foreword, Anthony Lewis criticizes Rehnquist for going back to first premises and for rethinking doctrine in terms of his personal constitutional ideology. Martin Shapiro, while seeing a new value of autonomy in Burger Court jurisprudence, argues that such values cannot explain doctrine, for "big values" such as autonomy and equality are not useful to Justices when they must make specific policy choices. The substantial gap between the findings of the scholars that, on the one hand, the Burger Court has recast the direction of law and, on the other hand, the failures of Lewis to understand the findings of the essays and of Blasi and Shapiro to explain rather than merely describe Burger Court findings, is due to a conceptual blinder that many legal scholars have: their refusal to see that Supreme Court Justices, all justices, have ideologies of rights and polity that shape their constitutional choices. Also one cannot explain in the Burger Court the presence of a value of autonomy and the recasting of judicial principles, so questions of polity, separation of powers, and federalism are manifest over rights concepts such as equality, without a reliance on concepts of ideology, of polity and rights, which are active across doctrinal areas. The cause of their failure to explain change between the Courts is a rejection of the idea that beliefs—ideologies about rights and polity—are normal in all justices. With such a belief, one which is supported by bloc and voting studies and systematic qualitative analysis of both Courts, we can get beyond ad hoc explanations of Court choices. Most significantly, we can move beyond a mere apology for what the Court does, because they did it: a problem that I thought we had met in the 1950s with the justified criticism of a social science based on the "End of Ideology," a faith in nominalism, and in bargaining, that is, marginal choices for their own sake.

RONALD KAHN

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The Formation of the Republican Party in New York: Politics and Conscience in the Antebellum North. By Hendrik Booraem V. (New York: New York University Press, 1983. Pp. 296. $39.50.)

Booraem has forsworn the easy and natural method of history—to understand the past as it understood itself—for the difficult task of understanding it better. He begins his work by denying any attempt at a "radical reinterpretation" of his subject. He ends with such a reinterpretation:

> the sudden upsurge of conscience politics in the North . . . stemmed mainly from a deep dissatis-

> faction among the voters with American society and particularly American politics—its corruption, its blather, its inflated promises, and its shoddy performance. If this is so . . . the politicians and the reformers . . . evaded the issue by attributing this discontent . . . to either of two distant enemies, the pope or the Slave Power. The energy of the voters was deflected into the anti-Catholic or anti-Southern crusades, and the shortcomings of democratic politics were not seriously addressed until the 1890s. (p. 224)

One would have thought that the presence in the United States of an entire class of persons stripped of all political rights, and virtually of all other rights, constituted a signal "short-coming of democratic politics," and that the formation of a national political party dedicated to stopping the expansion of this class of persons into new states —or rather the expansion of their masters—was a sufficiently serious method of addressing it. Not according to Booraem. He treats the fusion of Whigs, Republicans, dissident Democrats, and others into the Republican party in 1855 as an instance of the paranoid style in American politics. He could as easily be writing about McCarthyism.

The Republican argument, that free-soil was a constitutional mandate and thus a national interest whereas slavery was a mere sectional interest, is dismissed as a mere slogan, a "handy rejoinder," and "not a very convincing one when compared with the party's frequent fervic appeals to Northern pride and honor" (p. 123). Republicans frightened voters with the image of "some powerful, malign force," an "atrocious plot," a "dark and mysterious" Slave Power (p. 122). Never mind that Abraham Lincoln made this charge the keynote of the 1858 Illinois Senate campaign. The conspiracy charge was really a symbolic gesture, "infinitely serviceable" to Republican leaders because for believers, the immense battle for the salvation of the Republic left no room for any other issue or consideration" (p. 122). To stir up apocalyptic fears with the language of holy war, the New York Republican campaign of 1855 managed to bring "even moderate papers" to take seriously "the idea of a Southern conspiracy" (p. 122).

If the leaders of New York's fusion movement to combat the Slave Power were not serious about that issue, what was the motive for the creation of the Republican party? Booraem is more cautious in answering this question. He thinks that the classic historical treatments of this subject are approximately correct. However, they distort what actually occurred by placing too much emphasis on individuals and the "clash of ideologies." They forget the extent to which the aspirations of American politicians "have been balanced and, to some extent, formed by institutional needs; the views they have espoused have been shaped as much by the need for institutional consensus as by that for intellectual consistency or public appeal" (p. 2). He wants to remind us of the "institutional context."

In the present case this seems to refer to the infiltration of the Whig county organizations by members of the secret Know-Nothing societies. Booraem calls this an "immense threat" (p. 77). He says it supplied the motive for fusion which antislavery fervor could not (p. 70). Booraem also feels that the new Republicans were really the old Whigs in new bottles (pp. 80-81). He does not explain how such a change of name could overcome the infiltration of the party organization. And by dismissing the constitutional crisis that arose with the passage of the Kansas-Nebraska Act in 1854, he seems to foreclose the possibility of any rational explanation for the fusion convention of 1855.

Booraem has a nice analytic survey of the organization of the Whig party in New York in the early 1850s (chap. 3, especially pp. 83-108). One wishes he had complimented it by relating it to the Democratic organization and to the development of issues after 1844. But then that would be to take the leading figures of New York politics at face value, and to accept what they and their supporters thought to be the most urgent shortcomings of democratic politics for just that. It would be to "divorce political activity from its . . . institutional context" (p. 2); a mistake that is "easy and natural, . . . since the actors themselves, and their publics, commonly perceive politics in this fashion" (p. 1).

GEORGE FORSYTH

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The Legislative Veto: Congressional Control of Regulation. By Barbara Hinkson Craig. (Boulder, Colo.: Westview Press, 1983. Pp. xv + 176. $16.00.)

The legislative veto, recently declared unconstitutional by the Supreme Court, has been the subject of continual debate and controversy since its increased use by Congress in the 1970s. As a method of congressional oversight, the veto usually provides that either one or both houses of Congress, pursuant to statutory authority, can negate an action of the executive branch. Congress has enacted almost 200 legislative veto provisions into law since the veto's inception in 1932, and veto provisions appear in statutes such as those concerning arms sales, war powers, and home rule for the District of Columbia. In the 1970s Congress began applying the veto to agency rule-making.

Barbara Hinkson Craig's comprehensive work is an attempt to analyze through original case studies the veto's impact on the regulatory process and to assess its implications for the political system. Her thesis is that the legislative veto's value as a congressional tool to democratize decision making and resolve conflict within Congress is seriously deficient. Within the context of an analytical framework, four regulatory areas are chosen to test the thesis: the regulations of the Department of Housing and Urban Development, the Department of Education, the Federal Energy Regulatory Commission, and the National Highway and Traffic Safety Administration.

The analytical framework is based on Harold D. Lasswell, Morton Kaplan, and Theodore J. Lowi's "arenas of power" and is used to explore the nature of the interrelationships among political actors participating in the policymaking process. Within these arenas, policymaking relationships can be distributive, pluralistic, or administrative. Congress has transferred regulatory decision making to the administrative arena, intending to achieve a technologically and objectively sound policymaking process. The legislative veto was designed to provide a check on the excesses inherent in the enhancement of bureaucratic power and to open the decision-making process in order to make it more accountable and responsible. But as documented throughout the book, the legislative veto has promoted an administrative arena that is distributive, that is, closed. It has "instead forced regulatory policy decisions into subsystem politics, where pinpointing accountability or developing and pursuing broad social goals are nigh unto impossible" (p. 135).

Craig first finds that the legislative veto has not promoted democratic control of the rule-making process but has promoted clandestine policymaking in negotiations with the executive branch by unelected legislative (frequently subcommittee) staff, heavily influenced by special interest groups. Case studies of HUD's fair housing regulations and thermal insulation standards and the National Highway and Traffic Safety Administration's passive restraint rule illustrate this point. Often the vocal objections to proposed rules by individual legislators or staff members have blurred the objectivity of Congress as a whole, and the threat of the veto has "force[d] agencies to negotiate with subcommittee members and staff over the content of rules" (p. 145).

Furthermore, in terms of democratic theory, the inordinate weight of congressional staff in the policymaking process is especially troublesome. Congressional staff are subject to less control than executive bureaucrats whose rule-making is controlled by the Administrative Procedures Act and other requirements for procedural fairness.

Second, the legislative veto, according to Craig, has not ameliorated conflict within Congress on regulatory issues. When Congress cannot agree on a specific policy, the legislative veto allows Congress to avoid conflict by delegating the necessary flexibility to the executive branch, knowing that at a later date it can prohibit any delegation it believes to be improper. But Craig shows that the legislative veto has allowed Congress only to postpone inevitable decisions, that is postpone conflict, leaving agencies without congressional guidance and support for policy implementation. In the rule-making process, this practice causes delay, uncertainty, and confusion. But "the most common result [is] symbolic change and policy vacuum" (p. xvi). Examples illustrating this point include Title IX of the Education Amendments of 1974 and the legislative veto in the Natural Gas Policy Act.

Although Craig's use of the analytical framework sometimes becomes blurred by the excess of detail, this intensive, well-documented analysis is highly recommended reading. Although it was written before the Supreme Court's 1983 ruling of *Immigration and Naturalization Service* v. *Chadha* (103 S.Ct. 2764), which declared the legislative veto unconstitutional, the book has been updated to calculate the impact of that decision.

The evidence presented has borne out Craig's thesis and has provided a pragmatic basis for confirming the wisdom of the Court's ruling. Now it is time to encourage Congress to seek better solutions to deal with its regulatory dilemma.

LOUISE BYER MILLER

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Public Management Research in the United States. By G. David Garson and E. Samuel Overman. (New York: Praeger Publishers, 1983. Pp. v + 194. $24.95.)

Little has been done to actively support research on "the study of administration" since Woodrow Wilson wrote his famous article by that title nearly 100 years ago. This book offers empirical documentation of this problem by reporting the findings of a carefully designed, theory-directed survey of National Association of Schools of Public Affairs and Administration institutions. More than 250 research projects conducted from 1979 to 1982 were included in the project.

In this eight-chapter volume, Part 1 examines the need for meta-research of this kind and the study design. Part 2, consisting of chapters 3 to 5, includes an intellectual history of the discipline and the content and methodology of public management research. Those performing this work, their sponsors, and a concluding chapter on current trends comprise the final section. The survey reveals that there is "less than one funded public management project, of any type funded from any source, per public management program. . ." (p. 135).

The survey goes on to point out that "management of the research function in public administration is a myth. . . . Research producers must look to self-funding; funding through the applied concerns of governments; funding on the coattails of policy research; . . . or funding through scientific foundations oriented to traditional disciplinary priorities. . . . In this light it is hardly surprising that . . . there is so little public management research. . ." (p. 144). The cultural, political, and academic reasons for these findings are explored fully.

The contribution of this work is that it provides unique, baseline information on compensated research in the field. If, in the development of the sociology of knowledge in public administration, comparable future surveys are done, it will be possible to address a number of interesting issues such as the relationship of research to political priorities, management fads, curriculum development—indeed, the nature of the discipline itself. The authors suggest, however, that unless an NSF-like organization to promote administrative research is established, the research will remain fragmented, particularistic, and utilitarian.

Although few would argue that research on civil service systems is robust, one wonders if the state of that art is as deficient as the survey suggests. As Garson and Overman recognize, considering only funded research at universities limits their analysis. Financially supported research by contractors outside academe, as well as unfunded work inside its walls, constitutes a very significant portion of relevant research—no doubt substantially more than the 200-odd projects identified in the survey. In any case, this book, one of several by Garson and Overman, does not seem to provide the innovative information system to which it lays claim. Instead, it is more of an analysis of the problem than a user handbook for investigators. As a result, several major efforts at enhancing the bibliographic structure of the field by firms such as Dekker, Garland Publishing, Gale Publishing, and the Policy Studies Organization are overlooked.

Despite these problems, this is a useful volume for graduate courses on the state of the field. It maps the study of administration, explains trends, and offers concrete suggestions for improving the management and conduct of research. Garson and Overman's study deserves replication and expansion in the years ahead.

JAMES S. BOWMAN

Florida State University

The Politics of Ideas: Political Theory and American Public Policy. By Lawrence J. R. Herson. (Homewood, Ill.: Dorsey Press, 1984. Pp. xvii + 358. $14.95, paper.)

Incrementalists will have reason to rejoice with this volume, for it is written with Hersonian wit, wisdom, logic, humor, and poetry. It is a brilliant piece of scholarship, portraying in vivid terms the way in which American public policy is made: mutual adjustments among groups and leaders through the bargaining process, muddling through a Madisonian black box, coping with problems rather than solving them, and decision making through trial and error in a decentralized and fragmented system. Consequently, the public policy process produces outcomes that are piecemeal, indirect approaches to complex problems. Herson, however, rejects the concept that incremental policy is atheoretical because history is replete with examples of political ideas that have shaped and influenced American public policies.

Disappointment awaits those who are looking for a normative guide to public policymaking in this volume; Herson's theory of nondirect or incremental public policymaking is long on description but short on prescription. A survey of the literature on public policymaking in recent years shows that the incremental model of decision making through mutual adjustments has not escaped criticism. For one thing, bold and dynamic approaches may be preferable to small, piecemeal, and disjointed responses to societal demands for justice, especially from a group of people like the blacks who have suffered from oppressive policies for so long. For another thing, small and incremental policy changes may be much better than none at all, but it does not necessarily follow that such increments are much wiser and more prudent. They could also turn out to be increments to disaster, as the escalation of the Vietnam War policies of the past shows.

As a strategy of decision making, nondirect and incremental theory requires mutual adjustment of competing value orientations. Such an advice is useful, but only to those who have access to the public policymaking process and to those who have more power than others. Those who have no access at all will have no chance to share from the benefits of mutual adjustment. And even if access to the policymaking process is open to all, one must not forget the locus of power in any organization: It is unevenly distributed, so policy decisions will tend to be tainted with bias against the less powerful.

Incrementalism is remedial in nature in that it requires immediate feedback so that any error detected in the implementation process can be corrected. It tolerates mistakes and considers errors not as signals of failure but as opportunities for learning better ways of coping with problems. Indeed, evaluation is an essential aspect of the policy process. In order for it to be helpful, it must have adequate and relevant data about policy impacts. Unfortunately, this is not always the case. There are some public policies that have immediate societal impacts, and thus can be observed and measured. There are also some with sleeper effects, or time-delayed impacts which appear three or more decades later. The land reform policies of the New Deal in the South in the thirties are a good case in point. Policy impacts of this program upon black sharecroppers were not fully realized until the sixties. Early feedback indicated a policy failure because of some foreclosures. Incremental theory demands that remedial measures should have been initiated. Obviously, to have done so would have been premature.

Contrary to what some critics say, incremental theory encourages innovation and experimentation through trial and error and through a decentralized decision-making system. For example, instead of a centrally prescribed pollution control device to protect air quality, allowing coal-fired power plants to experiment with various ways of reducing sulfur dioxide emissions could lead to the discovery of a technological fixture that could reduce air pollution. On the other hand, if nuclear power plants are allowed to experiment with various methods of waste disposal and making safer and cheaper reactors, the cost would be prohibitive. If mistakes occur, as they undoubtedly will, the cost of their consequences will probably be more than the value of the resources allocated for the experiment.

Incremental and nondirect theory of public policymaking has its own merit. Like the moon, it has a bright side and a dark side. However, its bright side is not brighter than the light shone by more synoptic, comprehensive, centrally planned and directed systems. And its dark side is not as gloomy as its critics want us to believe.

A. B. VILLANUEVA

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Both Ends of the Avenue: The Presidency, the Executive Branch, and Congress in the 1980s. Edited by Anthony King. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1983. Pp. 273. $16.95, cloth; $8.95, paper.)

This book is about the relationship between two separate institutions, Congress and the presidency. The nine well-written chapters provide a thought-provoking look at how modern congressional-presidential relations have evolved and where they may be headed.

Nelson Polsby sketches historical moments in the development of institutional capabilities of the presidency and the Congress from the New Deal to the beginning of the Reagan administration. Polsby observes that over the last 50 years a presidential branch of government has emerged, separate from the executive branch. He contends that this development is responsible for the decline in the caliber of the career bureaucracy.

Hugh Heclo contributes an outstanding chapter examining the changed political environment for executive branch management and arguing that presidential strategies to deal with the new environment are undercutting the institutional capabilities of government. He discusses the first two years of President Reagan's management of the executive branch. Heclo predicts that there will be a search for new management devices that draw political institutions closer together.

Eric Davis takes a thorough look at changes in the White House congressional liaison function and personnel from Eisenhower through Carter. Davis observes that Reagan's relations with Congress should be relatively smooth, because many congressmen perceive that he has a mandate for conservative policies and because congressional-presidential institutional opposition is ending.

Charles Jones contrasts the styles and techniques for negotiating with Congress of Presidents Johnson, Nixon, Ford, and Carter. He concludes that a relationship dependent on the president, especially with high presidential turnover, is subject to dramatic change. Jones urges more research and analysis of presidential negotiations with Congress.

Austin Ranney analyzes the role of the modern political party as an institution that might enable the president to lead the Congress. Ranney finds it remarkable that with the weakness of political parties, presidents from 1953 to 1978 still received the support of members of their party in the House and Senate about two-thirds of the time.

Allen Schick presents an excellent analysis of congressional methods for imposing limitations on the executive branch. After a historical overview, he analyzes recent methods, including limitations in appropriation bills. Schick stresses the seesaw nature of relations between Congress and the president and observes that the president is beginning to ascend. This, he says, will mean fewer congressional limitations on Reagan's executive power.

Norman Ornstein contributes an outstanding description and analysis of changes in the internal operations of and outside influences on Congress during the 1960s and 1970s as compared to the 1950s. He demonstrates that Congress has become an "open system" where the norm is unpredictability. To prevail, Ornstein concludes, a president will have to know more congressmen and staff and deal with them more frequently, devote more time to this activity, and devise new ways to take advantage of the open system.

Michael Malbin presents an excellent analysis of key events related to President Carter's National Energy Plan. This case study explores presidential persuasion of the public and the Congress. Malbin observes that historically, successful presidential rhetoric has combined a specific and positive appeal to the public's character with a call for action.

Anthony King's outstanding concluding chapter argues that one of the main reasons that Congress and the president frequently come together is political pressure from the public. He emphasizes the importance of the "political climate" which is hard to define, but politicians know it when they feel it. He illustrates this concept with Reagan's early budget victories. King contends that political climate may be especially influential during times such as the early 1980s, when there is no generally accepted public philosophy.

Although some chapters could have been improved with more consideration of developments in congressional-presidential relations in 1981 and 1982, this book offers useful insights into the types of changes to expect in those relations during the 1980s.

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Dimensions of Tolerance: What Americans Believe about Civil Liberties. By Herbert McClosky and Alida Brill. (New York: Basic Books, 1983. Pp. x + 525. $29.95.)

There has of late been a resurgence of scholarly and practical interest in political tolerance, with *Dimensions of Tolerance* being the most recent entrant in the field. McClosky and Brill report on the analysis of a massive quantity of data on the beliefs of elites and masses in support of the basic thesis that Americans provide only tepid and conditional support for the rights of unpopular political minorities. This book will be recognized as an important contribution by those who care about democracy and liberty, as well as those who care about scholarly inquiry into democracy and liberty.

McClosky and Brill conceptualize political tolerance broadly to include attitudes toward the freedoms of speech (including symbolic speech), press, assembly, and religion, and the rights of due process, privacy, and lifestyles. Perhaps no other researchers propose such an expansive definition of tolerance, and it is necessary to keep this fact in mind in comparing this work to other research in the field. Roughly half of the book is devoted to examining levels of tolerance within these various domains. Although not too theoretically inspiring, this makes for fascinating descriptive reading.

McClosky and Brill offer a novel approach to the measurement of political tolerance and provide an enormous number of indicators for the various subdimensions of the concept. Their subjects are required to elect from alternative clauses to complete sentence stems. For example, they were asked to complete the stem "If a professor is suspected of spreading false ideas in his classes, college officials" with either "should not interfere since it would violate his rights" or "should send someone into his classes to check on him" or to check a "neither/undecided" response (p. 55). This approach has the substantial merit of reproducing a series of actual civil liberties conflicts in the questionnaire items (and it is carefully grounded in constitutional policy in the United States), thereby forcing subjects to weigh competing values in deriving their responses. But it also results in a great deal of missing data and may well have serious problems of validity and reliability. Moreover, the questionnaire is so long and so complex that a great deal of subject fatigue surely occurs. The scholarly reader will want to attend carefully to the quite important issues of measurement that are raised by this approach.

Beyond their contribution to conceptualizing and operationalizing political tolerance, McClosky and Brill advance an important and useful theory of the origin of tolerance. Grounded in a social learning framework not unlike that proposed by McClosky's student Paul Sniderman several years ago, they argue that tolerance is difficult to learn, requiring the repudiation, or at least harnessing, of the nearly innate tendency to distrust and fear what one cannot understand or control. Only the cognitively, ideologically, and politically sophisticated are successful at this task.

This assertion points to a third major contribution of the book: its pervasive concern for elite-mass and intraelite differences in political tolerance. Employing an innovative set of elite samples, McClosky and Brill carefully document the remarkable spread between elite and mass responses on virtually every single civil liberties issue. Moreover, these differences are shown to persist even under controls for a variety of demographic differences between elites and masses. But unlike many, McClosky and Brill also report substantial differences within their elite samples, typically ranging from the intolerant police officials to the very tolerant legal elites. The practice of treating elites as an undifferentiated democratic mass is no longer justifiable after this detailed analysis by McClosky and Brill.

In what is decidedly a microlevel treatise, one should perhaps not expect a great deal of concern over the consequences of citizen and leader intolerance. Yet especially in light of more general findings on the structure of beliefs and on the political consequences of opinion, one wonders whether responses on the myriad of issues raised by McClosky and Brill actually matter. That is, the strength of this book lies in its description of tolerance and its insights into the origin of tolerance. Its major weakness, a malady of nearly all tolerance research, is that it is inattentive to the complicated processes that link mass and elite opinion with political action, public policy, and ultimately, political freedom and democracy.

It has been 30 years since Samuel Stouffer conducted the first major tolerance survey, but in many respects the study of political tolerance is still in its infancy. Research like that reported by McClosky and Brill takes us many steps along the pathway to greater understanding of the causes and consequences of citizen beliefs on these important matters. Overall, their work is a formidable contribution.

JAMES L. GIBSON

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Regulation and the Courts: The Case of the Clean Air Act. By R. Shep Melnick. (Washington, D.C.: Brookings Institution, 1983. Pp. x + 404. $29.95, cloth; $11.95, paper.)

Whereas 30 years ago students of public law and policy more or less applauded judicial supervision of public institutions in order to ensure fair representation and equitable treatment of various interest groups, in the last decade there has been a growing concern about judicial intervention in regulatory politics as well as in other areas of public policy and administration. Court-watchers now question the capacity of the judiciary to contribute to the formulation and implementation of public policies and have begun to reassess the (anticipated and unanticipated) consequences of judicial intervention. Within this genre of scholarship, Shep Melnick's study of the Clean Air Act contributes much to an understanding of not only the transformation of the judicial/administrative partnership in regulatory politics, but also to the broader consequences of an expanded judicial role in American politics.

Rejecting the view that judicial expansionism attends "the growth of government," Melnick shares the perspective of other critics of "an imperial judiciary." In the area of health, safety, and environmental regulation, he asserts, "The courts did not follow, they led" (p. 4). In this well-written study, the consequences of reviewing courts for the execution of the Clean Air Act are explored in six areas and found to prove less than auspicious. As Melnick argues, "the consequences of court action under the Clean Air Act are neither random nor beneficial" (p. 344).

The consequences of judicial intervention are not random precisely because of the institutional structure and decentralized nature of the federal judiciary, the adjudicatory process, and judicial independence. In confining attention to individual disputes, the institutional structure of judicial decision making necessarily entails incremental, fragmented policy development. Although courts provide a forum for enhancing access and representation of interest groups in regulatory politics, decentralization within the judicial system promotes litigation strategies that further frustrate the development of coherent, unified policy. Regulated industries, for example, tend to adopt a rather defensive posture and challenge enforcement decisions in district courts, whereas environmentalists concentrate on the standard-setting process and thus litigate in appellate courts. District courts, in turn, tend to be more responsive to regulated interests, whereas appellate courts have broadened and even forced more rigorous standards in order to assure EPA's accountability and regulatory responsibilities. The latter have done so both because the adversary process invites a manipulation of "congressional intent" by judges—who, Melnick also contends, tend to rely on their own preconceptions about environmental issues—and because the process does not occasion judicial reflection on the feasibility and impact of particular rulings. Judicial independence proves no more advantageous, Melnick argues, because the isolation of judges has not inclined them "to take a broad view of the public interest" (p. 372).

Judicial review produces numerous unsalutory consequences for regulatory politics. In terms of the "success or failure" of policy, the institutional and procedural structure of courts conspire to exacerbate agency "muddling through" and further widen the gap between regulatory promise and performance. More importantly, Melnick suggests that the litigation strategies of interest groups and judicial intervention in regulatory policies results in a "court-committee symbiosis" (p. 376). To the extent that the position of subcommittee leaders and their staff, along with that of EPA officials—notably, in the Office of General Counsel—is thereby strengthened, the influence of the White House and Congress as a whole diminishes in the development of policy. The costs are considerable, Melnick concludes: "The courts have formed an integral part of a policymaking system that has bred cynicism, muddled debate, created serious inequities and inefficiencies, and kept pollution levels high in the most heavily populated areas of the country" (p. 387).

More than a detailed examination of judicial construction of the Clean Air Act, *Regulation and the Courts* contains provocative, although occasionally problematic, observations about the ascendance and consequences of an expanded judicial role in regulatory politics.

DAVID M. O'BRIEN

University of Virginia

The American People and Science Policy: The Role of Public Attitudes in the Policy Process. By Jon D. Miller. (New York: Pergamon Press, 1983. Pp. xvii + 145. $27.50.)

It is by now a cliche that virtually every major political issue on the national agenda has significant scientific components, that science policy itself is an important policy arena. The literature in specific science policy subareas is voluminous. Yet the intellectual domain of science, technology, and public policy is still a literature looking for a broader conceptual framework to bring coherence to the multiple questions that arise. Not since Don K. Price's *The Scientific Estate* (Harvard University Press, 1965) has a political scientist written a work that has attempted to do that for the issues, problems, and dynamics of science policy in the American political context. Miller's aim is "to examine and evaluate the structure and efficacy of public participation in the formulation of science policy in the United States" (p. 2). Chapter 1, "The Politics of Science," is introductory and three pages long. Chapter 2, "Science Policy in the Twentieth Century," provides a 17-page capsule history of American science policy that will not be very useful to those who are not already familiar with that history. The rest of the book is based on a series of NSF-sponsored surveys done from 1978 to 1981 by Miller and his associates. Data from an earlier 1957 study from the Survey Research Center at the University of Michigan are also utilized. The basic goal was to examine public attitudes toward, and participation in, science policy and to relate those findings to the general problem of citizen involvement in a democratic society.

Miller adopts the stratified model of policy formation that Gabriel Almond first developed in his study, *The American People and Foreign Policy* (Greenwood Press, 1950). That model typologizes citizens into a nonattentive public (at the base of the pyramid) which is ignorant, indifferent, and uninvolved; an interested public, aware but relatively uninformed on issues; an attentive public, comparatively involved, informed, and interested; a coterie of policy leaders (near the apex of the pyramid) which helps shape policy; and the actual decision makers.

The empirical data in *The American People and Science Policy* derive almost exclusively from the surveys, and it reinforces that earlier model with descriptions of the interest and issue specialization processes that are at work (chap. 4). About 30% of the adult population fits the classic picture of the democratic citizen, knowledgable about some issues and involved beyond the mere act of voting (p. 32). Chapter 5, "The Science Policy Agenda," reports on the findings of a 1981 leadership survey (281 scientific influentials). Among the six problems reported the level of funding—not surprisingly—was considered the most pressing. Viewed in order of decreasing importance were: the level of public understanding of science and technology, the quality of precollegiate science education, the obsolescence of research facilities, the availability of research and training opportunities for younger scientists, the level of incentives for industrial research and development, and the conversion of scientific knowledge into end products. Chapter 6, "The Acquisition of Resources," describes the leadership's priority concern with obtaining increased research support. Chapter 7, "The Preservation of Independence," concludes that "the basic compact between organized science and the government has been observed by both sides over the last four decades, and the level of anxiety appears to have diminished among science policy leaders" (p. 78). Chapter 8, "Public Participation in Specific Controversies," compares the responses of the nonattentive and attentive publics in the three issue areas of space exploration, nuclear power, and food additives. Chapter 9, "The Future of Public Participation in Science Policy," focuses on the process among young adults of issue attentiveness in general and attentiveness to science policy matters in particular. College-bound and educated respondents are better informed and more attentive. The concluding chapter, "The Formulation of Science Policy in a Democratic Society," recognizes the inherent tension between articulated democratic values and the stratified nature of public participation in science policymaking. Miller follows Almond's earlier conclusion that there is no inherent elitism in the way this tension works itself out in the American political system. In essence, issue specialization, issue fragmentation, issue diversity, and more generally, opportunities to affect outcomes, counter the potential for elitism. In short, pluralism is alive and well.

The strengths of this study are that it reports in great detail, and with many tables and charts, the empirical findings of the survey and analyzes them with skill and statistical sophistication. There is a wealth of data; much of it will be of interest to students of science policymaking. What would normally be a virtue—brevity—is, in this case, a weakness. The analysis of the broader issues is quite skimpy. Little effort is made to go beyond the data. This is a work that could, and should, have been elaborated on considerably. Moreover, the key issues revolving around the size of the unattentive public (the problem of scientific illiteracy for example) are taken as a given, as though in a modern democratic society both are an inevitable part of the political order. Pluralistic assumptions abound and are accepted uncritically. We will need to wait for a more elaborate, a more theoretically developed, and a more definitive work on science policymaking in America.

JOSEPH HABERER

Purdue University

The Hoover Commissions Revisited. By Ronald C. Moe. (Boulder, Colo.: Westview Press, 1982. Pp. xi + 138. $16.00, paper.)

This is an essential reference for those seeking a broad historical treatment of the two Hoover commissions. It is a thoroughly documented study, provided one pays attention to the footnotes and ignores a bibliography that does not contain many of the works cited in the end-of-chapter notes.

This is not, and does not pretend to be, an analytical study. It is a straightforward description of the personalities and activities of the two commissions. Hoover himself is, of course, the central figure, and one who is treated with scrupulous objectivity. Moe carefully documents the relatively greater success of the first commission, explains differences in political environments and personal relations that contributed to the differential success of the two commissions, and provides a fascinating description of the role played by the Citizens Committee—a lobby group dedicated to publicizing the commission's work.

Certainly one of the more interesting aspects of the work is Moe's treatment of the personal relationships that Hoover had with Presidents Truman and Eisenhower. He apparently had good rapport with Truman and shared a similar philosophy of government and administration. With the president of his own party, the situation was quite different. Eisenhower described Hoover's convictions as being "a trifle on the motheaten side" (p. 82), and throughout the life of the second commission was at pains to put himself at a distance from its work. This distancing of the president from an organization "outside the presidential orbit" (p. 81) certainly was a major factor contributing to the considerably diminished impact that the second commission was to have on governmental structure and organization.

As interesting and informative as *The Hoover Commissions Revisited* is, it suffers from a variety of minor problems. Often topics of some importance receive extraordinarily brief (one to three paragraphs) treatment (e.g., "Congressional Commissions," p. 8; the "Keep Commission" and the "Committee on Economy and Efficiency," p. 9). One is also intrigued at times by trivia as in, for example, the statement that "the second Hoover Commission was granted appropriations totaling $2,848,534. It expended $2,768,562 and returned $83,527 to the treasury" (p. 90). One wonders where the extra $3,555 came from. Or again, "The Commission found the Department of Justice conducted two business enterprises . . . [one of which] was the Federal Prison Industries, Inc." (p. 97). One wonders what the other business enterprise was.

The camera-ready format no doubt accounts for various slips from grace (e.g., "incessently," p. 108; "privitization," p. 48; "spirred others to action," p. 81; and "cannons of administrative orthodoxy," p. 106).

The bibliography, as noted above, is a curiosity. Truman's memoirs are included; Eisenhower's diaries (quoted in chapter 3) are not. Forty-nine books and articles, exclusive of government documents, are cited in chapter notes but are not included in the bibliography. Unfortunately, the price of the book possibly precludes its practical use.

Despite these flaws, the extensive documentation demonstrates thorough familiarity with the literature. Until Moe decides to write the definitive study, we must make do with this good beginning.

JOHN R. PHILLIPS

Thiel College

Legislature: California's School for Politics. By William K. Muir, Jr. (Chicago: University of Chicago Press, 1983. Pp. xiv + 219. $19.00.)

What would a good legislature be like? Muir has a convincing candidate: the California legislature in the mid 1970s, a few years after it earned the rank of best state lawmaking body in the nation and during a "golden age" of major policy accomplishments. On leave from the University of California at Berkeley, Muir served as a committee staff member in the California assembly in 1975 and 1976. From this experience and later interviews with legislators comes a picture—part description, part prescription—of legislative competence and the conditions that foster it. This is an elegant and absorbing work, of much greater value to our understanding of legislatures in democratic societies than the illustration of one body at one point in time would otherwise suggest.

Muir likens the good legislature to a school because it educates its members continuously in the "science of public policy and the arts of politics." This is not coercion but reciprocity, the voluntary exchange of knowledge among the members and others including lobbyists. California legislators were stimulated to become individually expert and proficient by several factors: (1) an open and independent committee system that taught policy expertise, (2) strong author control over legislation that taught such political skills as negotiation and persuasion, and (3) a legislative culture that encouraged members to become specialists in specific policy areas. The brighter and more hard-working members who developed policy specializations were those who made "a difference in the world," and they became teachers who willingly shared their knowledge with others. Aiding in this process was a solid support system of nonpartisan staff and various rules and procedures that aided open and comprehensive deliberation.

Why should elected representatives want to work so hard to achieve knowledge and expertise? One reason is ambition, the need of professional politicians to equip themselves for later stages in their public or private careers. Another is the desire for greater respect and influence among colleagues. But most important in Muir's scheme of incentives is a very basic assumption about human behavior, that people need to do "meaningful work" and to "avoid not mattering."

As thus portrayed, the forces that drive the good legislature are all internal—within the individual member and with the legislative institution in the interactions among members. Missing are the external connections, the effects of elections, and constituencies. Muir explicitly denies that democracy by itself produces competence and policy leadership; it was "good for responsiveness, but not for responsibility" (p. 183). Also problematic is the nature of the connection between process and product, although examples are given of far-reaching policies originated by the California legislature in the 1970s, many emulated by other states and even Congress.

Muir does not argue that legislatures in a democracy exist mainly to educate their members; some relationship between competence and doing good for the broader society is assumed. Nevertheless, there is very little here about how such linkages function between the internal and external worlds of the legislature. A lesser gap has to do with the more recent specifics of the California legislature, which by the early 1980s had lost a great deal of its collegial quality and spirit. Increased campaign funds, the bitterness of the post-Proposition 13 era, and new legislative infighting had taken their toll. Perhaps this is more properly the subject of another book, but the study of how legislative competence is lost should be considered as valuable as the study of how it is gained.

Overall, Muir tells us more about the purposes of legislatures—and especially about the quality institutions they can become—than the great majority of roll call, leadership, party, staffing, role behavior, and comparative studies that seem to dominate the literature on state legislatures. *Legislature* is written with clarity, great insight, and profound sensitivity to its subject. Closer in method and style to James D. Barber's *The Lawmakers* (Yale, 1965) than John C. Wahlke's *The Legislative System* (Wiley, 1962), it must be ranked with both of these works of the 1960s as a landmark.

ALVIN D. SOKOLOW

University of California, Davis

The Political Science of Criminal Justice. Edited by Stuart Nagel, Erika Fairchild, and Anthony Champagne. (Springfield, Ill.: Charles C. Thomas, 1983. Pp. xv + 288. $27.95.)

With the exception of appellate courts, the activities of criminal justice agencies are little studied or known by political scientists. This is a surprising neglect as criminal justice policies provide information, examples, and test cases for many of the defining theoretical concerns of political science, for example, the creation of law and laws that distribute the concrete application of such fundamental rights as liberty and justice among groups and individuals or the implementation of protective services at the local level by police organizations which exemplify the discretionary exercise of state power and the accountability of state agencies to public desires. Criminal justice policies, more than other acts of government, translate aspirations, ideals, and values that define styles and categories of polities into the dynamics of state-citizen linkages—the use of coercion against some to enforce conformity, the (unequal) provision of services that protect life and limb, or the "interested" exercise of discretion that can create or undermine legitimating notions. Life, liberty, and the pursuit of happiness is what criminal justice policies are all about.

The Political Science of Criminal Justice aspires to bring out the relevance of criminal justice issues and practices to political science, and the "special contributions of political science . . . [a perspective] that emphasizes who gets and who should get what, how, and why in governmental decision-making that relates to the formation and implementation of policies defining legal and illegal behavior" (p. ix) to the analysis of criminal justice policies. The book is organized into five sections that reflect the editors' understanding of how political science can be best brought to bear on the examination of criminal justice agencies and policies: political dynamics, constitutional constraints and Supreme Court impact, discretion and decision making, administrative efficiency, and policy evaluation. Each section is comprised of two or three articles and an introduction. The introductions are models of their kind; they succinctly and competently summarize and evaluate issues and some of the major works and findings in the literature. They will be extremely useful to political scientists who wish to gain a greater familiarity with the criminal justice field. The book is a well-organized, integrated, and useful anthology.

Individual chapters elaborate some of the issues raised in introductions. Articles in Section 1 discuss the influence of interest group actions on capital punishment legislation in California, the sentencing of draft resisters in federal courts, and on reforms of federal criminal codes. Articles in Section 2 deal with the impact of Supreme Court rulings on police performance and of federal courts on correctional policies in Dallas. Section 3 discusses factors that influence the exercise of discretion by police officers in encounters with citizens, by work-group participants in the processing of court cases, and by judges in bail and sentencing decisions. Section 4 analyzes the questions of whether small or large police departments provide better service, what political factors are related to a court's case load flows, and how budgetary constraints subvert the implementation of punitive correctional goals in Florida. Articles in Section 5 argue that presidential politicking affected the distribution of budgets among federal criminal justice agencies, that computer simulations may be usefully employed in judging the likely impact of decriminalizing marijuana possession on case loads, and that policy analytic thinking can bring out the costs and benefits of various approaches to the control of white-collar crime.

Articles achieve a generally high standard. They are theoretically informed, deal with the implications of case studies for larger issues, and substantiate their arguments by methodologically sound data. The data, though, aren't always analyzed as extensively as they could be.

By its very competence the book makes two unintended points, neither of which is really detrimental, but rather points out the virtues of interdisciplinary discourse. First, the political science label in the title, introductions, and articles is there mainly by courtesy and claim, not as a theme or specific conception. The definition of political science is much too loosely drawn to allow a thematic interpretation, nor are the sections organized by the conception which is offered. There is nothing particularly political science-like about the topics, methods, interpretations, or theory used in the articles; their contribution lies not in embodying a Lasswellian theme, but in being well done, in dealing with important questions in stimulating ways. Second, the articles highlight the irrelevance of academic protectionism, the argument that certain topics and subjects are suitable primarily for, therefore reserved to, particular orientations. The analysis of criminal justice requires a variety of frameworks; it is a subject too important to be left to specialists or allocated to particular perspectives.

OTWIN MARENIN

University of Baltimore

Parties, Elections, and Representation in the State of New York. By Howard A. Scarrow. (New York: New York University Press, 1983. Pp. ix + 135. $25.00, cloth; $12.50, paper.)

This short book provides an overview of the topics included in its title and will prove useful primarily to readers who generally are familiar with the political system in New York State. With the exceptions of two chapters which are reprinted in part from other publications, Scarrow's book contains relatively little original research data and analysis.

Scarrow notes three major changes in the state's political party system: the decline of the Republican party and the rise of the Democratic party in terms of voter registration, the increase in the number of independent voters, and "the elimination of the bipolarity which once existed between the parties' enrollment strengths, respectively, in New York City and the remainder of the state" (p. 10). However, in discussing control of New York City by Democrats Scarrow fails to explain how it is possible for Republicans to elect five or six state senators in the city. And the section on roll-call analysis is flawed by failure to explain "empty chair" voting in the state senate.

The sections on the impact of the state's 1967 challenge primary law, cross-endorsements, and third-party politics generally are good explanations of relatively complex topics. Scarrow concludes "that the minor parties have, on the whole, lived up to their proclaimed objective of backing legislators of the appropriate ideological leanings" (p. 70).

Scarrow feels that a major perspective guiding the analysis "is comparison with other American States. . ." (p. vii). Unfortunately, the references to other states are relatively few in number and scattered.

Although Scarrow portrays many of the dynamic changes that have occurred in primary and general election campaigns including the 1982 gubernatorial campaign of Lewis Lehrman, no mention is made of the 1976 United States Supreme Court decision—*Buckley* v. *Valeo* (424 U.S. 1)—which made possible a Lehrman-type candidacy.

Party raids by Vito Marcantonio of the American Labor party in 1942 dismayed Democratic and Republican party leaders. Scarrow suggests that the raids induced the state legislature to amend the election law to make party raiding more difficult but fails to explain why the legislature waited five years after the raid to amend the law.

The failure to provide citations to all laws and other published materials referred to is a weakness of the book, as is Scarrow's reliance upon secondary sources of information when primary sources are readily available.

In sum, the sparsity of literature on political parties in New York State makes the book a welcome addition, but the need for a major book on political parties, elections, and representation in the state remains.

JOSEPH F. ZIMMERMAN

State University of New York at Albany

Majority Leadership in the U.S. House. By Barbara Sinclair. (Baltimore: Johns Hopkins University Press, 1983. Pp. 336. $25.00, cloth; $12.95, paper.)

Barbara Sinclair's book is essential reading for all students of Congress and will find a place in most courses on Congress. Her basic argument is important, straightforward, and convincing: Party leaders have two major goals, building winning coalitions and keeping peace in the party. These goals sometimes reinforce each other and sometimes work at odds to each other. Changes in leaders' working environment during the 1970s made the achievement of both goals even more difficult. Lacking sufficient resources to manage their decentralized chamber composed of independent, activist members, the O'Neill/Wright team has adopted three strategies: provide services to members, structure legislative options carefully, and include as many Democrats as possible in the coalition-building process. By these strategies, the leadership permits members to pursue their individual goals, minimizes the need for coercion, gains additional assistance, and hopefully instills an appreciation for leaders' predicament in the rank-and-file. As successful as these strategies have been, however, the key determinants of leadership success, party size and cohesiveness and the nature of the policy agenda, remain beyond leaders' control. Parts of Sinclair's argument will be familiar to those who have read her articles, but it is more fully developed and illustrated in this book.

Sinclair served in the House majority leader's office as an APSA congressional fellow in 1978 and 1979. Her position gave her access to meetings of the Democratic Caucus, the Steering and Policy Committee, the whip organization and other leadership meetings, to the House floor, to confidential documents, and to participants to whom only to or three other political scientists in recent decades had access. With her unique vantage point and through follow-up interviews, Sinclair is able to provide the first in-depth look at the organization and operation of the Democratic leadership in the "post-reform" House. She supports her argument with excellent examples and participants' views throughout the book and provides six tightly written case studies of leadership coalition-building efforts in the 1979 to 1981 period. As a result, the book serves as a good reference on House leadership, as a lively introduction to the dynamic environment and roles of party leaders, and as a solid contribution to the scholarly literature on congressional leadership.

Beyond the rich description of leadership operations and strategies, Sinclair's major contribution is her discussion of leaders' goals. In the mold of Richard Fenno's treatment of committee members' goals, Sinclair presents leaders' goals in a manner that makes her analysis both meaningful to participants and useful to the political scientist. She takes us beyond the laundry lists of leadership functions, roles, and activities that have dominated discussions of party leadership by carefully examining the interaction between the two goals and their implications for leaders' ability to serve the needs of rank-and-file members.

Sinclair also advances efforts to systematically assess leadership success in Congress. With quantitative and qualitative analyses of leadership task force efforts to build coalitions, voting patterns on roll calls recognized by the leadership as important to them, and overall leadership success in shaping and passing legislation, she provides one of the most direct and meaningful evaluations of party leadership activity available. Only an analysis of whip polls could provide a more complete picture of leadership success.

This book has few limitations of either omission or commission. But there are a few.

The most obvious is that the interaction among Democratic party leaders is given only peripheral treatment. This is a very sensitive subject, of course, and so it is difficult for the outsider to observe and even more difficult to report on objectively. Nevertheless, it is a subject essential to a discussion of leaders' goals. For example, the Speaker and Majority Leader do not have identical political interests, even though their goals are usually compatible. These differences often lead the top two leaders to play different roles, and, perhaps more importantly, lead rank-and-file members to deal with the leaders in different ways.

Sinclair sometimes overstates how neatly leaders' tactics fit their general strategies and goals. She develops a picture of a leadership that has adapted deliberately and brilliantly to its circumstances; its failures are entirely due to factors outside of its control. The problem stems, at times, from her willingness to accept the official explanation at face value. The inclusion of four major committee chairmen on the Steering and Policy Committee, for example, is described as an element of the strategy of inclusion. In fact, O'Neill was seeking a way to keep Richard Bolling on the committee after his term expired without alienating other major committee chairmen. Developments in leadership strategy and operations also are often more evolutionary than Sinclair suggests; approaches that work continue to be employed, with their broader rationale developed only over time.

Unfortunately, descriptive material in a book of this type often becomes outdated quickly. For example, the use of task forces to solicit support and the use of the Steering and Policy staff to communicate with committees have not been as common in the last two congresses as they were in O'Neill's first two terms in the speakership. Nevertheless, the book will be the definitive description of House majority leadership under Speaker O'Neill.

These limitations detract very little, however, from an excellent book on party leadership. We can only hope that other scholars will produce similar treatments of the other three sets of congressional party leaders in the near future.

STEVEN S. SMITH

Northwestern University

The Other Candidates: Third Parties in Presidential Elections. By Frank Smallwood. (Hanover, N.J.: University Press of New England, 1983. Pp. xvi + 317. $20.00, cloth; $9.95, paper.)

Smallwood records his interviews with the 11 "third party" presidential candidates who garnered over seven million votes in 1980. He delves into their personal backgrounds, reasons for running, party philosophies, and obstacles they confronted in getting onto the ballots.

The interviews are framed between chapters on the two-party monopoly and the impact and future of third parties. These review essays are quite readable and should prove useful to students and teachers of American government. Although these chapters do not break any new ground, they pose thoughtful questions for our political system. Are the barriers to third-party participation too high? How much access can we provide (should we provide) for the views of third-party candidates without overloading the system?

The interviews are fascinating. In their own words, the third-party candidates come across overall as reasonable, rational people with points of view worth hearing about. They hold no illusions about winning. They run because it is the only way to get a single issue discussed (Ellen McCormack, Right-To-Life), to build membership (Socialists and parties of the Left generally), and to declaim views and ideas the major parties will not touch.

In reading the interviews, one is admonished that the pictures in our heads are incomplete, often out of focus. For example, candidates of the Right are the most emphatic that the United States cannot be policeman for the world (American party and the American Independent party). The Socialist candidates explain the consuming ideological conflicts—among their pacifists and hawks, liberal and conservative factions—that render the movement overall as inept, rather pathetic.

All the candidates decry the obstacles they faced in getting onto the ballots. Yet two made it in all 50 states (Ed Clark, Libertarian, and Independent John Anderson), whereas Barry Commoner and the Citizens party were represented in 30. Several states had 10 parties on the ballot.

The candidates also lament the lack of national media attention: plenty of local coverage, little from the networks and national newspapers. This is an important issue. As we have seen in the major party presidential primaries, a select group of columnists, reporters, and assignment editors serve as the gatekeepers who allow or deny access into the national consciousness. Should perceived potential for winning be the only criterion for access?

Smallwood was a stand-in running mate in 1980 with John Anderson in Vermont to fulfill ballot requirements in that state. Thus it is not surprising that his analysis reflects the frustrations faced by third-party candidates. The Federal Election Campaign Act (FECA) is criticized repeatedly for having a major party bias, especially with regard to funding. One could argue, however, that FECA provides third parties a golden opportunity to achieve a critical objective in party-building, that is, significant funding, by winning only 5% of the national vote.

There should be copies of this well-written book in every college and high school library, as it is a natural resource for term papers. Smallwood stimulates questions about our political system that can be discussed at several levels of sophistication. What responsibilities, if any, do our governmental and media institutions have to help market ideas and political platforms? Why do so many Americans seem to crave candidates who are different and yet fear ideas that are different?

JAMES D. NOWLAN

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The Social Transformation of American Medicine. By Paul Starr. (New York: Basic Books, 1982. Pp. xi + 514. $24.95, cloth; $11.95, paper.)

Rising health care costs have long been an issue for national debate, and the health care crisis has become a familiar part of our political agenda. Both liberals and conservatives have applied different forms of regulatory and economic control without particular success. Costs have continued to escalate, unresponsive to political or social intervention. The problems of health care delivery are frequently viewed one dimensionally: monochrome villains are discovered, and overly simplistic solutions are set forth, with a predictable lack of success. Thus, it is the particular merit of Paul Starr's comprehensive treatise that the many facets and the evolving complexity of America's health care system are presented in depth in a form that is both scholarly and readable.

A sociologist, Starr has produced a work whose approach goes beyond the confines of any single discipline or point of view; his book provides a critical, historical perspective of American medical care, with careful attention to the social and political dynamics of the contending groups: physicians, unions, hospital administrators, reformers, insurers, and consumers. The first part of the book traces the evolution of medicine from a relatively unimportant profession in the early nineteenth century to the position of nearly absolute sovereignty that it attained by the middle of the twentieth. In 1869, when the benefits of medical treatment were negligible, a medical journal observed that "medicine has ever been, and is now, the most despised of all the professions" (p. 7). As anesthesia made surgery possible, and as antitoxins provided dramatic cures, hospitals were transformed into other than merely custodians of the dying. Medicine became respectable and powerful, and physicians became the unchallenged stewards of its practice, arbiters of the political and social structure of the health care system.

A vast industry grew up around medicine: hospitals, drug companies, and manufacturers of equipment and supplies all shared in the expansion. America became a country dominated by large corporations, yet physicians avoided the corporate structure, and severely punished members of the medical fraternity who attempted to adopt it. Social experimentation in the financing of medical care and in restructuring access to the care system was severely limited by the power of physicians, who were united in their opposition to virtually any form of external regulation. Starr presents fascinating accounts of the battles between social reformers who wished to broaden access to care and physicians who managed to control the forces of change and shape these forces to their liking.

The last chapters of the book are perhaps the most pertinent to current political thought. In the face of new technology, higher wages for hospital workers, massive, unregulated hospital construction, and the extension of care to the elderly and the indigent through federal social programs, the overall costs of medical care had risen by late 1982 to approximately 10.5% of GNP. This increase, progressing relentlessly over the last 35 years, has been finally sufficient to call forth the intervention of regulators, and one mechanism after another has been tried in attempts to limit the increase. In the last few years, political forces have emerged that are drastically reshaping the health industry. Cost reimbursement is being replaced by competition, prospective payment, and an absolute cap on the dollars spent for health reimbursement. This is stimulating the rapid growth of large corporate health polyconglomerates, both for profit and not-for-profit institutions. I am not sanguine that this new mode will provide a stable structure consistent with the highest quality medical care system, but a corporate-based health care system has already emerged as the pattern. Starr's book will be an invaluable source of both information and insight for all of those who are concerned about these changes.

ALLAN H. LEVY

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Energy Decision Making: The Interaction of Law and Policy. By Joseph P. Tomain and Sheila S. Hollis. (Lexington, Mass.: D.C. Heath, 1983. Pp. xvi + 203. $24.95.)

In the last 10 years there has been an abundant production of books purporting to analyze U.S. energy problems and policy issues. Some have focused on the technical and economic aspects of energy supply and consumption, some on the environmental consequences of energy development and use, and a fair number on political and administrative issues, the last presumably of greatest interest to political scientists. Given the volume of work on the subject and the high quality of many recent efforts to assess energy politics, one expects much of a new book with the title *Energy Decision Making.* Although this volume offers a range of insights into legal and administrative procedures affecting policy decisions, political scientists specializing in energy policy are likely to be disappointed.

According to the authors, the purpose of the book is to study "how the federal government makes decisions that affect the allocation and distribution of energy resources in times of scarcity or plenty" (p. xiv). In particular, they set out to examine the underlying administrative and legal structure that they argue continues regardless of who occupies the White House. The topics addressed reflect the authors' backgrounds in energy law and their interest in administrative procedures and the role of the courts. There are 10 chapters covering the basics of administrative law, judicial review of agency actions, regulatory reform, the impact of federalism on energy decision making, rate-making processes, cost-benefit analysis, and enforcement decision making among others. They are arranged into three major sections: the institutional structure, decision-making methodologies, and the values embodied in those institutional structures and methodologies and thus reflected in the decisions made. The overall organization of the book makes sense, but the chapters are not particularly well-integrated, and there is some repetition throughout. A stronger unifying theme would have been helpful to tie together what are often quite disparate discussions of energy decision making in different settings.

Given the focus and legal orientation of this book, it would be difficult to characterize it as a major contribution to the energy policy literature. Reference to energy policy and politics is infrequent, with most citations covering only court cases and administrative law. Moreover, the analysis offered is neither systematic nor theoretically informed. Despite those limitations, the descriptive accounts of energy decision making are up to date and presented clearly. Thus, readers may find perusal of selected chapters valuable, particularly if a brief introduction meets their needs. The cases, observations, and conclusions offered will be of interest to a broad audience concerned with the way administrative procedures and methodologies such as cost-benefit analysis affect energy decision making.

Curiously, Tomain and Hollis conclude that energy policy changes "take place within a narrow range of options" imposed by the administrative structure they describe. They suggest that the institutional apparatus is so well-entrenched that it "will exist independently of the policy preferences of either [Carter or Reagan]." Thus, they assert that neither could dramatically affect the environment within which energy decisions are made. One is hard pressed to accept such a conclusion given the dramatic shift in energy policy within the Reagan administration, particularly because it was accomplished in large part through an administrative strategy that reorganized offices and agencies and significantly affected policy implementation in the Department of Energy and the Department of the Interior through drastic budget cuts and ideologically driven personnel policies.

Finally, it is worth noting that Tomain and Hollis state in the last paragraph in the book that value issues "are at the heart of energy decisions," are often submerged in the administrative apparatus, and severely limit the development of a national energy policy. Rather than referring to the extensive political literature that explores those concerns, they end with the hope that decision makers at all levels will make their values explicit in the future. Given the significant omissions in this book, one should supplement it with a sampling of some of the better volumes that have emerged in the last several years on the politics of energy (e.g., works by Lawrence, Axelrod, Goodwin, Kash and Rycroft, and Rosenbaum, among others). The interaction of law and policy is important, but as the authors themselves concede, political judgments are also highly significant.

MICHAEL E. KRAFT

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Scarce Natural Resources: The Challenge to Public Policymaking. Edited by Susan Welch and Robert Miewald. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. i + 288. $25.00, cloth; $12.50, paper.)

This anthology, Volume 11 of the Sage "Yearbooks in Politics and Public Policy," brings together a group of scholars whose interest is to explore the public policy implications of natural resource scarcity. The general focus of the book is upon the value issues implicit in any policy response to scarcity. Not as theoretically rigorous as the recent Resources for the Future anthology edited by V. K. Smith (*Scarcity and Growth Reconsidered,* Johns Hopkins University Press, 1979), the editors expressly eschew arguments about economic efficiency in favor of questioning the underlying political assumptions and values of those persons entrusted to manage natural resources in an age of scarcity.

The book is divided into two parts, 13 chapters, with an introductory chapter by the editors. The first part deals with a variety of theoretical approaches to resource policymaking. As is usually the case with anthologies, the eight chapters of Part 1 vary in quality and readability. Several chapters in this section deal directly or indirectly with various aspects of William Ophul's work (*Ecology and the Politics of Scarcity,* Freeman, 1977), with R. McGreggor Cawley making the most spirited case against Ophul's prediction that only a new Leviathan can deal with resource scarcities. Cawley makes an interesting argument that Locke, not Hobbes, offers a political philosophy more compatible with Ophul's desire for a steady-state society. Jeanne Nienaber's chapter outlining historical responses to perceived scarcity by American governments and bureaucracies is a useful antidote to a certain apocalyptic flavor evident in some of the recent literature on scarcity. Susan Rigdon's review of resource policies in communist states, and Lester W. Milbrath's analysis of public sentiments about resource scarcity in four capitalist countries, make a useful contrast and introduce a comparative orientation too often lacking in this literature.

Part 2 introduces five case studies of resource policymaking. These are of mixed quality, and as is again usual in anthologies, the cases only infrequently carry over the major themes of the theoretical chapters. The best of the five are Uday Desai and Osbin L. Irwin's study of coal severance taxes, Allen R. Wilcox's analysis of public lands policies and the "Sagebrush Rebellion," and William P. Browne and Kenneth J. Meier's review of soil conservation and the agricultural lobby. The chapter by Michael W. Bowers and Dorotha M. Bradley on water scarcity in Arizona most directly carries on the discussion of Ophuls, but the chapter—written by graduate students at the University of Arizona—still needs better focus and editing.

There is an air of nostalgia about this book. In a decade scared of declining industrial productivity, the struggle between "sunrise" and "sunset" industries, and foreign competition, *Scarce Natural Resources* deals with issues that were current during the oil crisis of 10 years ago. It is not that scarcity issues have lost their relevance—they have simply been pushed offstage by other worries. What is lacking in this book, and what would have made it a major contribution to the ongoing debate, is any discussion of the impact of these newer concerns upon scarcity issues. For example, there are substantial political problems, of both theory and practice, in asking an unemployed, middle-aged, under-educated, mortgaged former steel worker to think about—much less willingly accept—irreversible declines in his life-style in the name of a steady-state society. The political pressures brought by the legions of such people make taking the kinds of steps needed to achieve that society ever more politically impossible. How to get a society bent upon regaining its former economic vigor to think in other, more ecological, terms is the real crux of the 1980s and 1990s' political problems. Unfortunately, these problems are not addressed in this book.

PATRICK W. HAMLETT

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All That Is Native and Fine: The Politics of Culture in an American Region. By David E. Whisnant. (Chapel Hill: University of North Carolina Press, 1983. Pp. xvi + 340. $24.00.)

This important book examines three cases of extended cultural missionary work in Appalachia: the Hindman Settlement School (founded 1902), the efforts of Olive Dame Campbell (who worked with British ballad collector Cecil Sharp and helped found the John C. Campbell Folk School in 1925), and the White Top Folk Festival (1931-1939). In each case outside intervenors—upper-middle-class New Englanders or aristocratic Southerners—attempted to preserve and aid what they perceived to be traditional mountain culture. Yet according to David Whisnant, these apparently disinterested outsiders did as much to undermine traditional culture as to save it. Cultural workers came to Appalachia with preconceptions of what constituted authentic folk culture: In particular, they sought survivals of English ballads, dances, and crafts that would demonstrate the Anglo-Saxon rootedness of American culture and which, preserved and revived, could serve as a bulwark against the mongrolizing effects of other popular traditions. Such notions led them to sanction only those aspects of mountain culture of which they approved, and even to introduce traits they fancied to belong there, while discouraging everything else—from mountaineers' adaptations of jazz to their ongoing creation of songs about the latter-day realities of coal mining, labor protest, and feuding. And because the outsiders represented a dominant class, their definitions of authentic culture were often accepted as authoritative by mountaineers, who then became the bearers of newly created traditions.

Beyond his extensive documentation of the fabrication of "traditional" mountain culture, Whisnant argues that a narrow focus on culture allowed the missionaries to avoid any involvement in the harsh sociopolitical realities of Appalachia, where "economic colonization by northeastern capital" led to "indigenous resistance" and sometimes to violence (p. 6). Often the capitalists funded cultural workers, who in any case viewed the education of individuals rather than communal protest and organized labor as the answer to Appalachian problems. In sum, Whisnant believes that the work of cultural missionaries involved manipulation and sometimes exploitation: Not only did they impose their genteel standards upon mountaineers whose hardships they often ignored, but they also established themselves as cultural arbiters and mediators as, for example, in their control of handicrafts associations marketing mountaineers' products to the outside world. Unfortunately, this aspect of Whisnant's argument suffers from a lack of attention to the social-structural details of exploitation: His persistently expressed belief that cultural workers should have engaged themselves politically is rarely argued from the facts of the cases. For example, he devotes little attention to the impact of schools and festivals on local communities except to claim that they failed to do anything other than facilitate individual mobility. This may be true, yet it needs to be argued thoroughly in order to justify the moral condemnation built into Whisnant's "historical judgment" (p. 263).

Moreover, although Whisnant exposes the inauthentic, stereotypical Appalachian culture constructed by those seeking "pure" traditions, he himself clings to a notion of purity in speaking of the "indigenous cultural tradition" of the folk (p. 175). This is all the more surprising in that Whisnant argues that "cultural intervention" is a "feature of every cultural past, . . . present and future" (p. 254). Such a claim suggests that "pure" culture never exists, even though Whisnant tries to find it by espousing the view of the downtrodden. In the end it is ironic that Whisnant condemns certain efforts aimed at improving the economic situation of indigenous artisans—for example, introducing Scandinavian designs in order to market handicrafts more successfully—on the grounds that the results were inauthentic. In sum, Whisnant clings to a notion of cultural purity, defined in terms of class or community autonomy, while rejecting imposed innovations as polluting and exploitative. But this conflates notions of exploitation and indigenousness that Whisnant should have developed more carefully in what is otherwise a fine book.

RICHARD HANDLER

Lake Forest College

The Fractured Electorate: Political Parties and Social Change in Southern New England. By John Kenneth White. (Hanover, N.H.: University Press of New England, 1983. Pp. xii + 159. $16.00, cloth; $7.95, paper.)

John White's *The Fractured Electorate,* an analysis of the relationship between social-cultural change and change in party coalitions in Massachusetts, Rhode Island, and Connecticut, represents the increasing interest in partisan change in the northeastern states. White's analysis is based on more than 50 open-ended interviews with party officials, office holders, and business leaders, as well as poll data and election statistics from a variety of sources.

In the first two chapters the three great "tides" or political-issue agendas of this century are outlined. The first was an ethnocultural tide that was characterized by conflict between Catholics and Protestants over issues such as immigration policy and prohibition. The second was the "New Deal Tide" that involved conflict concerning government involvement in the economy. The third tide, a social and cultural tide, represents the emergence of a new "postindustrial" economic order. The old order is represented by the still-dominant but declining group of non-college educated, blue-collar workers, whereas the new order is composed of college trained professionals. These two groups serve as the basis of conflict over a new issue agenda that includes abortion rights, the death penalty, homosexual rights, divorce, and protecting the environment.

In the fourth and fifth chapters the impact of the social-cultural tide on New England party coalitions is examined. The Democrats in Southern New England have become "everyone's party"; young affluent professionals have increasingly identified with the Democrats. The result has been a great deal of intraparty conflict between the new professionals and the blue-collar old guard. Campaigns have been characterized by extreme conflict over social issues or a complete lack of discussion of issues. The Republicans, on the other hand, are disappearing; they have done little to appeal to the new professional class. Republican candidates deemphasize their party affiliation, and what is left of Republican party organization serves as a stage for conflict between pragmatists and purists.

In the final chapter White gives his attention to the implications of the decline of the two-party system in Southern New England and the propensity for conflict to be staged outside of the party arena. He endorses the Connecticut "challenge primary" as one way to begin restoration of the parties. The concluding chapter is followed by a postscript on the 1982 elections.

The argument in this volume is compelling, especially in light of recent Southern New England presidential primary struggles. However, those who are familiar with recent research on southern politics by such scholars as Numan V. Bartley and Hugh D. Graham (*Southern Politics and the Second Reconstruction,* Johns Hopkins University Press, 1975) will find this book disappointing. Two aspects of White's analysis are especially troublesome. First, there is little indication that White has touched base with a great deal of the political science literature on dealignment/realignment. Second, the presentation of evidence in support of the book's thesis is unconvincing; White relies too heavily on statements made by party officials and journalists. Analysis of data derived from polls is superficial. Given some of the data cited, an opportunity to provide strong statistical support for the book's argument was missed.

This is an interesting book, but it does not measure up to many other subnational studies that have been reported in recent years.

JAMES M. CARLSON

Providence College

Comparative and Other Area Studies

Von Armenhaus zum Wohlfahrtsstaat: Analysen zur Entwicklung der Sozialversicherung in Westeuropa. By Jens Alber. (Frankfurt: Campus Verlag, 1982. Pp. 280. DM 48, paper.)

Having grown to critical proportions throughout Western Europe since they were first introduced 100 years ago in Bismarck's Germany, welfare systems are in retrenchment today. The huge social expenditures—composed primarily of generous pensions, health care, educational benefits, and unemployment payments—are being subjected to cutbacks. It is a time of growing concern over the future of the welfare state, which makes the appearance of Jens Alber's comparative analysis of the genesis and expansion of the welfare systems of Western Europe seem highly appropriate. The book is the revised version of Alber's doctoral dissertation and was done at the University of Mannheim in connection with a larger research project, "Historical Indicators of the Western European Democracies."

The work deals with the origin and development of the social security systems of 15 Western European countries. It begins with a discussion of the emergence of the social programs in the context of the formidable structural changes of nineteenth-century European society. Alber devotes much space to a survey of the existing literature on the origin and development of the welfare state. Contrasting Marxist and pluralist models, he isolates a number of hypotheses which he intends to test. The following analyses address questions pertaining to the causes and the extent of the social security systems in Western Europe. Data are assembled in tables throughout the text and in an extensive appendix. They are assessed rigorously for principal determinants of the origin and growth of the welfare state. What was the relative impact of socioeconomic pressure versus party political struggles, the demands of mobilized workers versus the initiatives of conservative elites? Additionally, Alber seeks information on the importance of such factors as external influences and the particular type of governmental system. Although the social security programs were introduced in times of intensifying social problems brought about by the industrial revolution, it is not certain whether it was a deeper need for welfare provisions, or simply the greater availability of resources, which led to their establishment.

Alber's efforts yield some interesting information. For example, he finds that the *level* of industrialization and urbanization is not an important determinant. In fact, the countries lagging behind in these respects tended to have more programs than those leading. Neither is there a significant link between the development of labor organizations and the extent of the social security programs. Nevertheless, the two start together, and the growing political solidarity of the workers may have induced the powerholders to implement social legislation "from above." Alber demonstrates the close relationship between social security initiatives and political legitimation efforts. In the creation of social security systems, the more authoritarian regimes are ahead of the more democratic ones, the economically less-developed countries ahead of the more developed ones. Potentially destabilizing elements of society are integrated through paternalism and centralization. As to the phenomenal growth of the welfare state since World War II, the data seem to suggest that the cause was mostly political expediency, rather than the dictates of socioeconomic conditions. Once the extensive programs were in place, the substantial demographic shifts required their further expansion, and the social expenditures would vastly increase with the enormous rise in the number of eligible service recipients.

Given the growing political leverage of groups receiving assistance in one form or another, the assumption that no let-up in welfarism can be expected easily suggests itself. But, as noted at the outset, current developments seem to prove otherwise. Governments throughout Western Europe —with Helmut Kohl and Margaret Thatcher setting the pace—are trimming their social spending. Nonetheless, Alber has contributed to an understanding of the growth and the causes of the welfare state. It is a well-executed study, likely to be of considerable use to readers of German with a special interest in the field of comparative public policy.

MANFRED GROTE

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Implementing Development Assistance: European Approaches to Basic Needs. By Steven H. Arnold. (Boulder, Colo.: Westview Press, 1982. Pp. xvi + 220. $20.00.)

Students of international aid take note: The United States is no longer the only significant contributor to third-world development. Steven H. Arnold's new study of the European aid community draws our attention to the role of a new set of states that have filled the gaps in the structure of international assistance created by the exit of the most powerful participant. Given the size of the American economy, the United States remains the single largest donor country, but the Europeans as a group provide more aid to the Third World in the 1980s than both the United States and OPEC countries combined. United States' aid declined from 60% of total international assistance in 1960 to 20% in 1980. In the wake of a shrinking assistance commitment from the United States, Europe became the Third World's alternative to a decrease in overall national development expenditure. Whether it has become an alternative to U.S. leadership in the areas of policy innovation and operating procedures is not quite so clear. Arnold's book is a rather good discussion of the ideological, administrative, and programmatic differences that characterize the aid programs of five European states. The intent is to "reveal a rich variety of national styles and technical approaches" (p. xv) and to explore the complexity of the assistance task itself. In fact, what emerges is a story of missed opportunities. European leadership in the sphere of international aid has gone the way of mutually exclusive national goals and an overattention to domestic economic benefits and bureaucracies. Further, those states offering a global vision of the role of assistance in the process of third-world development are the weakest members of the European aid phalanx.

Arnold makes France, Britain, Germany, Sweden, and the Netherlands the focus of his anatomy of European aid. He looks at the first three because they have consistently been the most important European donors, and the last two because their aid programs have grown so rapidly. There are no real surprises in Arnold's analysis —France is most committed to the diffusion of culture, Germany to the transfer of technology, Sweden to the export of social democracy—but the juxtaposition of country programs allows us to see how firmly planted each is in its own national context. France is perhaps the most self-interested of the five. Almost 60% of its aid goes to upper and upper-middle income countries. Almost as much is tied to the purchase of goods in France. Its staff is drawn from former colonial officers. Its venture into assistance to the least-developed world is part of an effort to create colonial client states in West and Central Africa. Its capabilities allow France to play the aid game with a degree of seriousness not available to weaker European participants. In terms of size of annual assistance budgets, field staff, and bureaucratic clout, the French are in the position of the front runner. Close behind are the British and the Germans. Both states have sizeable aid budgets, but Arnold calls the German program "blurred," and it appears that the British also lack a clear image as a donor state. Both carefully attend to the economic and financial benefits of assistance. Despite the fact that Germany does not formally tie its aid, 80% of all German aid is spent on German goods. Britain is currently in the process of radical aid cutbacks. The German bureaucracy is at war with itself on aid ideology, and the British simply fail to articulate one. Both embrace a commitment to reaching the least-developed countries, but neither has developed a project design or program focus to do so systematically.

In contrast, the efforts of both the Netherlands and Sweden are suffused with a political commitment to making aid serve the interests of recipients as well as donor states. Further, both have experimented with new ideas and new modes of assistance programming and implementation. With considerable popular support in both cases, the Dutch and the Swedes charted a basic needs approach at the same time—if not before—the World Bank and the United States began to redirect their own assistance programs. Their aid commitments as a proportion of GNP are amongst the highest—the Dutch at 1%. Their attempts to reach the least-developed are expressed in terms of programs in the areas of agriculture, health, and social services. Their links to private NGO's are established in order to find alternative avenues for reaching into third-world societies. But these are the very states without the economic capabilities to make their aid vision a basis for European leadership.

The lack of consensus and competitive programs of the Europeans can be multiplied many times over with the addition of the other bilateral and multilateral aid actors who compose the international aid community as a whole. Arnold's conclusion in respect to the separate directions of European actors and the very different *modus operandi* of each is even more daunting when we consider that this is but a microcosm of the international system as a whole. International aid needs to be rationalized—Habermas or Huntington, take your pick. Arnold's study clearly accomplishes the goals he sets for himself and provides a basis for creative comparing within Europe. It also points to a policy agenda in respect to third-world development assistance that is beyond the resurrection of a hegemon and towards the establishment of a global aid regime.

CONSTANCE G. ANTHONY

Boston College

The Sociology of the State. By Bertrand Badie and Pierre Birnbaum. Translated by Arthur Goldhammer. (Chicago: University of Chicago Press, 1983. Pp. x + 171. $24.00, cloth; $10.95, paper.)

Bertrand Badie and Pierre Birnbaum have written a sophisticated critique of both modernization theory, which holds that the modern state is completely autonomous, and instrumental Marxism, which holds that the state in industrialized societies is, except for rare and temporary interludes, dependent on the capitalist class. Arguing that the "true state" should be narrowly defined and distinguished from the broader notion of a centralized political system, these two French authors conclude that the bureaucratic state of France closely approximates the true state: "one that has achieved a certain level of differentiation, autonomy, universality, and institutionalization" (p. 60). But this true state, even though it is largely independent of the ruling class (p. 119), is not considered ideal by the authors, either as a standard for evaluating the degree of rationalization in other societies or as a developmental goal to be exported to the Third World. Although they are writing from within the functionalist tradition (p. 49), the authors agree with Marxists that modernization theory is fundamentally ideological and in error in holding up the Western state as the inevitable result of social differentiation and rationalization. Rejecting the inevitability orientations of both functionalists and Marxists, Badie and Birnbaum see the state as a somewhat unique social invention that was devised to solve specific crises in certain Western European societies at a particular point in their historical development.

Although Badie and Birnbaum agree that social differentiation growing out of an increasingly complex division of labor does create problems of societal coordination, they do not believe that the state in the narrow sense is the only social device that can supply rational coordination (p. 65). Citing the examples of both Great Britain and the United States with their *weak* states, Badie and Birnbaum argue that the coordination can be provided by the presence of a political center. Britain and the United States lack a true state but do have political centers, whereas culturally divided Italy lacks a political center but has a strong state. Modern France has both a true state and a political center, but Switzerland has neither (p. 63). In societies lacking a strong state, civil society coordinates itself, which means that the market and the business class dominate the state as they do in the United States and Great Britain. In contrast, the self-reproducing, meritocratically recruited French bureaucracy is largely independent of the business class. Thus, contrary to both modernization theorists and instrumental Marxists, the state in advanced capitalist societies is neither inevitably autonomous nor automatically dependent on the capitalist class.

Despite the balanced analysis and the judicious use of Marxist and non-Marxist scholarship in this clearly and concisely written book, some problems remain. In the first place, the concept of the political center is admittedly vaguely defined (p. 56). Worse, it is not consistently applied: Usually the term is used to refer to the presence of a unified political community or culture (pp. 63, 93, 105), but at times it is used to refer to any central government that lacks the dominant bureaucracy of the true state (pp. 54, 83), and at other times it seems to have no clear referent at all (pp. 49, 59-60). In the second place, the analysis of the effect that religious differences have had on the rise of the state is flawed. Protestantism is supposed to lead to a weak state, but the glaring failure of that effect in authoritarian Prussia and pre-nineteenth-century Sweden is not acknowledged (p. 88), hence never adequately explained. Finally, the conclusion that the business class dominates the British and American states, but not the French state, is tarnished by the authors' tendency to cite only supportive sections of studies rather than also demonstrating the methodological or empirical shortcomings of contrary interpretations of power.

EDWARD S. MALECKI

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State and Conflict in the Middle East: Emergence of the Postcolonial State. By Gabriel Ben-Dor. (New York: Praeger Publishers, 1983. Pp. xiii + 271. $29.95.)

Notwithstanding the activities of non-state actors and the challenges of suprastate ideologies, the state has proven to be an enduring structure in the Middle East. This ordinary conclusion is the starting point for Gabriel Ben-Dor, who argues that the growing supremacy of the state is not only an empirical reality, but also provides hope that the triumph of the state heralds an era of realism and even peace in the region.

Ben-Dor's conceptual focus is the quality of "stateness." Although never clearly defined, the reader infers that stateness is a measure of the functional breadth, sovereignty, domestic autonomy, and sociocultural authenticity of a given state. Contrary to our sometimes deformed appreciation of Islam, Ben-Dor correctly demonstrates that "state[ness] and Islam are not contradictory in either logical or empirical terms" (p. 49). Indeed, contemporary manifestations of militant Islamic activism are notable for the extent to which they strive to attain political power *qua* actors in existing states. Such conclusions are not unique to Ben-Dor, however. The point was made with customary eloquence by Fouad Ajami in his much acclaimed article, "The End of Pan-Arabism" (*Foreign Affairs,* winter 1978/1979, *57,* 355-373), which is repeatedly cited in *State and Conflict in the Middle East.*

In his most interesting chapter, "Stateness, Zionism, and Israel," Ben-Dor offers a paradoxical analysis of the waning primacy of *raison d'etat* for Israel. Arguably the two most powerful political personalities in the modern Middle East were Gamal Abdul Nasser and Ben-Gurion. When Nasser died in 1970, the already faded dream of Arab nationalism was buried, with therapeutic effect for the Arab states that had long suffered the debilitating legitimacy challenge of pan-Arabism. But even as Nasserism was waning, Ben-Dor argues, Ben-Gurion's cold-blooded commitment to *raison d'etat* was rejected by his political heirs (especially Moshe Dayan) and his antagonists, as embodied by Menachem Begin. Initially it was the 1967 victory that "shattered the status quo, and revived serious debate on the most fundamental existential questions concerning the Israeli state" (p. 115). But it was Begin's electoral victory in 1977 that signalled the decisive choice: "caliphate" in lieu of state. The one quality that epitomizes *raison d'etat,* pragmatism, was supplanted by dogmatism.

Although it is fair to infer that an Israel—led by Begin's disciples and motivated by Revisionist Zionism—is unlikely to improve the chances for peace in the Middle East, Ben-Dor does not convince his reader that the enhancement of stateness (either in Israel or the Arab states) would lead to sanguine outcomes. His assertion that "Strong Arab states may potentially pose a more serious challenge to Israel on the battlefield, but they are less likely to get there in the first place, because for them balance of power, *raison d'etat,* and conflict as competition by nonviolent means with states pursuing their interests are more meaningful concepts of statecraft than are weak counterparts permeated with the logic of nationalism and revolution" (p. 214) is certainly—at least—problematic. Furthermore, Ben-Dor's previous espousal of a political culture approach to the region would itself seem to counsel a certain skepticism toward any such analysis (see the *International Journal of Middle East Studies,* 1977, *8,* 43-63). As Ajami warns: "Normalization of the Arab system, on the whole positive and overdue, brings in its train its own troubles, inflicts its own wounds, commits its own errors" ("Pan-Arabism," p. 373).

Gabriel Ben-Dor has in other forums demonstrated a keen mind and a serious commitment to melding social science with Middle Eastern studies—an all too rare combination. However, in *State and Conflict* he has submerged a number of interesting, if controversial, points in a sea of turgid prose, unnecessarily long quotations, and unevenly reasoned arguments. It is, in short, a book-length journal article; I would have preferred the condensed version.

AUGUSTUS RICHARD NORTON

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The End and the Beginning: The Nicaraguan Revolution. By John A. Booth. (Boulder, Colo.: Westview Press, 1982. Pp. vii + 279. $25.00, cloth; $11.50, paper.)

Nicaragua in Revolution. Edited by Thomas W. Walker. (New York: Praeger Publishers, 1982. Pp. iii + 410. $32.95, cloth; $14.95, paper.)

The Nicaraguan revolution has inspired the sympathetic academic efforts of United States scholars as well as the open hostility of governmental policymakers. These two volumes represent the best of the emerging scholarship in the United States on the early years of the Nicaraguan revolution. John Booth's *The End and the Beginning* provides a fascinating analysis of the historical conditions leading to the 1979 Sandinista victory that ended 46 years of Somoza family rule. Booth identifies the roots of Nicaragua's revolution in the nation's history of political factionalism, its geographic position, its economic dependence on the United States, and patterns of class conflict within the nation. Successive chapters treat the early years of domestic partisan strife and United States' intervention, the formation of the Somoza dynasty, and Augusto César Sandino's legacy of resistance. The economic, political, and social control exercised by the Somoza family is carefully analyzed, giving the reader an understanding of the dynasty's roots. Booth makes his greatest contribution to understanding the forces behind the Nicaraguan revolution in his analysis of the development of a broad-based opposition coalition to Somoza. Booth's discussion of the alienation and radicalization of groups as diverse as labor, the Church, and the upper class is extremely well done and makes for fascinating reading. The month-by-month account of the insurrection itself transports the reader back to the days of the military struggle and serves as a basis for understanding the fervor behind the revolution.

Booth's assessment of the Nicaraguan revolution is incorporated in the book's title, and the beginning of the revolution is analyzed in terms of the revolutionary government's initial years in office. Reflecting its mid-1982 publication date, the book's treatment of the revolutionary regime is largely confined to a summary of governmental goals and initial actions. With the benefit of hindsight, one can see the roots of the government's later problems in this chapter, particularly the difficulties of developing policies toward indigenous groups, tensions between church and state, economic difficulties in consolidating revolutionary plans, differences within the governing coalition, and difficult relations with the United States and its neighbors.

Although the book begins and ends with a discussion of the applicability of theories of revolution to the Nicaraguan case, the chapters are all too brief in their coverage of theoretical issues. Booth outlines the causes of the Nicaraguan revolution in terms of relative deprivation and specific revolutionary catalysts, and ultimately ascribes the success of the insurrection to a change in the "coercive balance," the relative strength of the regime and opposition, from the early 1970s on. Although the somewhat cursory treatment of theoretical considerations limits the study's wider utility, all in all, *The End and the Beginning* presents a fascinating overview of the dynamics of the insurrection resulting in the 1979 Sandinista victory.

Booth's study analyzes the historic development of the Nicaraguan insurrection, whereas *Nicaragua in Revolution,* edited by Thomas Walker, provides a more comprehensive and a more contemporary analysis of the revolutionary process itself. The 21 chapters by an impressive array of contributors examine most of the major forces behind the Nicaraguan revolution. Following a brief introduction in which Walker establishes a historical context for the revolution, three authors examine the insurrection itself: Ricardo Chavarria analyzes the insurrection in the context of Latin American experiences with revolution, Harry Vanden explores the revolutionary ideology, and William LeoGrande examines United States-Nicaraguan relations throughout the revolution. Walker's impressionistic recollections of his spur-of-the-moment trip to Nicaragua in the days following the insurrection provides a very human, first-hand account of the heady days after Somoza's overthrow. The volume's principal focus lies in its comprehensive treatment of the forces involved in the Nicaraguan insurrection and in the creation of a revolutionary society. Chapters explore the role of the Sandinista mass organizations (Luis Serra), the armed forces (Stephen M. Gorman), working-class organizations (Marifeli Perez-Stable), women (Susan Ramirez-Horton), the Church (Michael Dodson and T. S. Montgomery), and the news media (John Spicer Nichols). The policies of the revolutionary government are discussed in detail with chapters on economic policy (E. V. K. FitzGerald), agrarian reform (David Kaimowitz and Joseph R. Thome), literacy campaigns (Valerie Miller), health policy (Thomas John Bossert), housing (Harvey Williams), sports (Eric A. Wagner), indigenous minorities (Philippe Bourgois), and foreign policy (Alejandro Bendaña). The concluding section establishes an international context for understanding the revolution with chapters on the Costa Rican role in the Sandinista victory (Mitchell A. Seligson and William J. Carroll), Cuban perspectives (Max Azicri), and the reemerging cold war (Susanne Jonas).

The chapters are of uniformly high quality both in terms of their scholarly contribution and their readability. Unfortunately some of the chapters are already dated. FitzGerald's very capable analysis of the first year of the revolutionary government's economic programs, Bendaña's description of the regime's foreign policy, and Gorman's discussion of the military forces in the first year of the revolution provide few clues into the nature of the intense conflicts that would develop in these areas in the years following the book's publication. However, this shortcoming is perhaps inevitable given the difficulties inherent in writing about a revolution in process.

Both works underscore the dynamic nature of the revolutionary process and the difficulties in translating insurrectionary fervor into revolutionary policies. The two works are essentially complementary, with Booth's study providing a depth of understanding through its historical focus, whereas the edited volume by Walker provides a breadth of coverage through its analysis of a wide range of contemporary forces and issues. Together they provide a relatively complete view of the early years of Nicaragua's revolution. Definitive analyses of the Nicaraguan revolution will, of course, have to await the outcome of the revolutionary process.

ELIZABETH G. FERRIS

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Metropolitan Winnipeg: Politics and Reform of Local Government. By Meyer Brownstone and T. J. Plunkett. (Berkeley: University of California Press, 1983. Pp. xxxii + 230. $35.00.)

This is a complete accounting of the extensive reorganization undergone by the city of Winnipeg in the early 1970s which led to what has come to be known as the "unicity" or single-city government scheme. This is in contrast to the two-tier structure that characterizes metropolitan government in many other centers of Canada, most notably Toronto, Montreal, and Edmonton. Brownstone and Plunkett were both intimately involved in the planning and decision-making leading to the amalgamation of Greater Winnipeg, the city itself, and 11 suburbs into a new unified City of Winnipeg, and their narrative deals primarily with the special issues involved in the effort.

The authors are systematic: They first elaborate on the traditions of local government in Canada and then discuss the Winnipeg political and historical environment leading to change, the movement from general Manitoba provincial government policy to actual legislation, the major issues and their resolution, the transition to the new "unicity" system and its full implementation, the experience with the new system and recent amendments to it, and, finally, conclude with two chapters, an analysis of the reform process in Winnipeg and an assessment of the new system in terms of decision making.

In effect, the study picks up the Winnipeg situation where one section of Harold Kaplan's immense *Reform Planning and City Politics: Montreal, Winnipeg, Toronto* (Toronto, 1982) reviewed in the *American Political Science Review* (1983, *77,* 504) left it and brings it more or less up to date.

Historians of urban political reform will find the detailed completeness of this study especially valuable, whereas Canadianists will be interested in the recounting of the interplay of Manitoba provincial and Winnipeg local pressures. As Brownstone and Plunkett note, the freedom of Canadian local politicians for maneuvering is far more constrained than that of their American counterparts, because final decisions in urban politics lie constitutionally with the provincial governments. No American state governor or legislature has anything approaching the influence of a provincial government, and American local politics appears far more disorderly as a result.

At the same time, although the authors do place what took place in Winnipeg within the context of Canadian experience in local and municipal government, there is not enough generalization to make this work, interesting although it may be, much more than a recounting of a particularistic set of incidents.

However, there is a foreword some 14 pages long written by the publications editor and co-editor of the Lane Fund, which sponsored this volume, that urbanologists will find useful. It points out that the study touches on four issues about which specialists in urban politics have yet to achieve a consensus and gives each a concise but focused airing. These questions are about the problem of representation. First, what is the "best" form of representation and how can local community and neighborhood participation be encouraged? Second, why is there still in the North American urban setting a persistent and continued "denial of politics"? Third, how can municipalities deal globally with issues while still retaining local representation? And fourth, should the municipal chief executive be elected directly or indirectly?

Although this study deals extensively with Winnipeg, its interest to students of local politics is likely to be in the light the narrative can shed on these questions.

PETER REGENSTREIF

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The Falklands Crisis: The Rights and Wrongs. By Peter Calvert. (New York: St. Martin's Press, 1982. Pp. x + 183. $20.00.)

Besides serving as a convenient testing ground for the latest trends in conventional military warfare, the 45-day Falkland Islands conflict spawned a veritable cottage industry of *ex post facto* analyses probing the causes and consequences of the dispute. Most of these analyses fall into two categories: those that examine the legal background and diplomatic negotiations before and during the crisis, and those that address the military and/or political lessons learned. As is the case with this brief volume, some ambitious studies endeavor to do both. However, whereas appraisals of the military lessons have been fairly consistent, positions on the legal and diplomatic questions have tended to diverge along idiomatic lines. With the exception of Argentine authors writing in English, most works in that language have sided with the British claim to the islands. Conversely, Spanish-speaking authors have overwhelmingly backed the Argentine claim.

In many respects, this book is both a case in point and an exception to the rule. Although his support for the British position is apparent from the onset (the first chapter is dedicated to reaffirming standard British arguments on the subject), Calvert, a Reader in Politics at the University of Southampton and inveterate Latin America-watcher, nonetheless takes pains to demonstrate the complex historical and environmental factors leading up to the exchange of blows, as well as British errors that contributed to the armed conflict with Argentina. Although he favors self-determination for the "kelpers" and views Argentina as the aggressor in 1982, Calvert capably demonstrates how misperceptions on both sides helped escalate the dispute towards its ultimate denouement.

Originally published in England, the book is obviously designed for domestic consumption. There is a remarkable paucity of non-British sources of documentation. The vast majority of the sources cited consist of British government reports and newspaper stories. The few non-British works used are mostly historical references. Although this does not prevent strong presentation of the forces at play in British decision-making circles immediately before and during the crisis, it leaves his analysis of Argentine motivations and historical perspective rather thin.

The chapter on Argentina contains several factual errors that may well be a product of over-reliance on secondary sources coupled with ignorance of Argentine history. Elsewhere, Calvert's logic seems muddled. For example, when attempting to substantiate the preeminence of British claims to the islands over those of the Spanish (from whom the Argentines claim to have inherited their rights to the islands), he resorts to the patently untenable notion that "the absence of a Spanish name for the islands is the clearest possible proof that they were neither discovered nor settled in the first instance by Spain" (p. 5).

The book does, however, have some very strong points. Particularly good are Calvert's descriptions of events leading up to and through the Argentine occupation and subsequent surrender of the islands, of the internal debate that shaped the form and eventual logistics of the British response, and of the frenetic diplomatic maneuverings that ultimately failed to avert bloodshed. Equally valuable are his accounts of the role anticolonialism played in shifting international opinion away from Britain (which made a decisive military victory all the more necessary) and of initial U.S. ambivalence over the part it would play in the affair.

Although Calvert's review of the British campaign is adequate, the summary of military lessons learned is otherwise perfunctory, virtually blind to the Argentine side of the story, and little more than a rehash of evaluations offered elsewhere. And while his belief that British intra-service cooperation won out over Argentine internal dissent is essentially correct, another statement—that the Falklands conflict demonstrates the diminished viability of Britain's nuclear deterrent (p. 158)—is overdrawn.

Calvert maintains that the principal political lesson to be derived from the crisis is that intransigence and aggressiveness pay off. Britain won, he argues, because it was both intransigent on the issue of sovereignty during negotiations (despite some internal misgivings) and more aggressive when waging war. Argentina scored diplomatic victories with its own intransigence on the sovereignty issue, but lost the war because of a lack of military aggressiveness, the heroics of its aviation notwithstanding. Although the latter point is true in both instances, the former can be disputed, since it was intransigence, after all, that brought these nations to a war neither wanted.

As a chronicle of events, the book deserves high marks. As an analytic construct for evaluating the crisis and its after effects, it is less praiseworthy. This unevenness hurts the overall quality of the book, and its orientation towards a British audience mitigates against acceptance on a broader level. Even so, this volume represents an "enlightened" British view of what is clearly a sensitive national issue. For all its pro-British tone, it recognizes errors in British as well as Argentine decision making and strives for neutrality when describing the course of events during the crisis. For a polarized body of literature, this is no mean feat and a definite step forward.

PAUL G. BUCHANAN

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An Anatomy of Ghanaian Politics: Managing Political Recession, 1969-82. By Naomi Chazan. (Boulder, Colo.: Westview Press, 1983. Pp. xvii + 429. $27.50.)

This is one of those books that, although of some utility, is made difficult because of an unduly convoluted style of presentation and a need for editing. Perhaps the most glaring editorial problem is Chazan's tendency to reduce almost every point to three, four, or five categories. This ubiquitous pattern—two, three, or more times in virtually every chapter—seriously detracts from clarity. Other technical problems emerge when substantive concepts are presented imprecisely. Two examples are outstanding. In chapter 5 on leadership, Chazan undermines her argument by comparing two quite difference phenomena, whereas other, more useful options are available. (Leaders of the chieftancy are more usefully compared with local leaders affiliated with the state rather than national leaders.) Moreover, it would be much more effective in chapter 6 to exercise greater choice about actors critical to the "fall" in Ghana, as opposed to rehashing eclectically four or five existent models. Problems such as these combine to undermine Chazan's overall efforts. On the one hand, the perspective is correct that Ghana has gone from its lofty focus on social change (expansion of the political community to the masses) to a condition of mere survival (with a radically deteriorated state and resource level). But on the other hand, when all of the analysis is taken together, one is not entirely satisfied that the evidence has been marshaled in the most effective manner to prove the case.

The problems begin with Chazan's two variable lists. She separates what are defined as state institutions and the nonformal sector. The separation makes it appear that the variables are not in fact linked and do not influence each other, albeit in complicated ways. Chazan's point is that there is a state structure in one part of the firmament, and the people are in quite another. This is so because the state sector as originally conceived was not designed to respond to the sentiments and welfare interests of the nonformal sector. As time has passed, the state has not achieved symmetry with, or even responsiveness to, the nonformal sector. In order for this argument to work, it seems that Chazan would have to assign the state only broker functions, to respond to claims here and there. There would be little opportunity for a state to have independent, collective purposes as might be required by an ideology. Such a scheme seems too simple. Of course there is a divide in Ghana and elsewhere. The problem is to explain the complicated interactions between people in the two sectors. In this circumstance someone is gaining, or losing, or attempting to maintain the status quo; there are interactions and choices that sustain such involved relationships.

The book is organized into four sections, in only one of which (Part 3) I found substantially new material. The first part considers the political structure in Ghana, whereas the second treats the processes of politics. Part 3 takes up the last 15 years from the Second Republic. The fourth part considers prospects for Ghana's future. Much of the analysis along the way is rich and detailed. There is a good discussion on the foundations of Ghanaian politics and society. Chazan makes an effective distinction between the vertical and horizontal entities to the foundation, although much of the case seems to go against the grain of the two variable lists guiding the analysis. At the same time, too little attention is given to the way in which dynamics structure contemporary factions in contradistinction to continuities from traditional organizations.

Finally, the most successful and most interesting parts of this analysis are those in Part 3 between chapters 7 and 10. Chazan is essentially on her own in interpreting data derived from her own research in the field and reading. She presents fresh information that has not been pulled together in this way before and is able to make reasoned interpretations of use to scholars of contemporary Ghanaian politics. One only wishes that there had been more analysis of this material and that all of it had been presented in this section rather than scattered elsewhere in the book.

MINION KC MORRISON

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The GDR: Moscow's German Ally. By David Childs. (Winchester, Mass.: George Allen & Unwin, 1983. Pp. xiii + 346. $27.50, cloth; $14.50, paper.)

David Childs, a Reader in Politics at Nottingham University in England, presents a comprehensive, very detailed description of the political evolution of the German Democratic Republic since its inception in 1949. He also covers economic and relevant social aspects including education, the nature of the mass media, and the rather successful struggle for women's rights in the East German society.

The treatment of the subject matter is almost encyclopedic. A very large number of individuals and events are mentioned or briefly described. Most of them are likely to be significant only to those who already have an extensive knowledge of the GDR. On the other hand, the average layman interested in East Germany and the Soviet bloc may be almost overwhelmed by the mass of information presented, although brief episodes inserted into the text from time to time help to enliven the pages.

Although the vast data presented reflect Childs's deep knowledge of, and close familiarity with, all aspects of GDR history and current conditions, both domestic and international, it is curious that he does not end his book with a chapter offering a summary and conclusions as well as projections for the future. Eastern Europe suffers from both economic malaise and political ferment, and therefore an appropriate concluding chapter to a treatise regarding such an important actor as the GDR within the Soviet bloc could have provided interesting insights into the operation of a regional system that critically affects the welfare of Western Europe and the United States. Moreover, a more systematic approach to the analysis of the comparative figures presented by Childs in several chapters of the book (for example chapters 6 and 10) may have contributed to greater conceptual focus. Finally, the interrelationship between German-German and East German-Soviet interactions needs to be analyzed more carefully, especially the leverage which an obedient GDR may have on Soviet East-West policies.

After all, the GDR regime does have a clear economic interest in good relations with the Federal Republic, and this relationship is affected by the nature of the relationship between Moscow and Washington. Although the whole book deals with the links between the GDR and the Soviet Union, only two and a half pages are devoted to a systematic treatment of this relationship (pp. 308-310).

In spite of these critical comments, Childs's book provides a valuable addition to our knowledge of the GDR and should be required reading for all those who have a major interest in Eastern Europe and the Soviet bloc.

WERNER J. FELD

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Politics, Policy and the European Recession. Edited by Andrew Cox. (New York: St. Martin's Press, 1982. Pp. ix + 267. $25.00.)

This book gives an overview of the various ways in which the European Community and the major European countries—West Germany, Sweden, the Netherlands, France, Britain, Italy, and Spain —responded to the severe recessions of the 1970s and early 1980s. Seven of the authors are British political scientists, one is an economic historian, and one is a Dutch professor of social and economic policy.

That the recessions of 1974 to 1975 and 1980 to 1981 were the most serious economic setbacks for the industrial world since the 1930s makes an examination of the relationships between the politics and economics of this period intrinsically worthwhile. The book devotes a chapter to each country listed previously and to the European Community itself. Each chapter is primarily a description of the economic policymaking institutions in each country and the EC and a discussion of how those institutions responded to the economic difficulties experienced since the 1974 oil shock.

Cox makes some effort to integrate the various country studies into a whole, but the book would have been even more useful for building theory about the relationships between politics and economics if it had offered a common set of political-economic concepts and a set of hypotheses to be tested systematically with the evidence contained in the book. The hypotheses are there but are embedded implicitly—and not always obviously—in the discussion. For example, Kenneth Dyson says of Germany: "the balance of party political power was clearly significant for the evolution of economic policy" (p. 59). That is, political power that was fragmented between the SPD and their FDP coalition partners led to a need for accommodation in policymaking. Such implicit hypotheses abound throughout the book, but it would be useful for them to be summarized and made more explicit.

The major conclusion of the book seems to be that "those nations which have developed incorporatist approaches to the recession have fared much better than those nations in which fundamental political cleavages remain to be resolved" (p. 29). That is, the strong corporatist structures of consultation and decision making in Sweden and West Germany appear to have played a major role in producing the particular economic outcomes of high growth and low unemployment that those countries experienced in the 1970s. In contrast, strongly adversarial relations between management and labor that government was not able or willing to overcome help to explain the poor economic performance of Britain, Italy, and Spain. The authors cannot really be faulted for not pursuing the more basic question of whether corporatist-consensual institutions of decision making are mainly a cause or an effect of economic growth. That is, perhaps Sweden's consensual policymaking style resulted from wealth, not merely produced wealth (i.e., economic growth), and Spain's adversarial policymaking style resulted from that country's poverty. The relationship between economic and political variables is almost certainly circular rather than one-directional.

Although it does not organize the material in as theoretically systematic a way as one might wish, the book, along with several others of recent years, could provide much of the raw material for constructing and testing a theory of capitalist political economy, especially that part of a theory dealing with the effectiveness of various public policy instruments for pursuing economic growth, high employment, and price stability. The experience of most of the countries discussed in *Politics, Policy and the European Recession* seems to confirm Andrew Shonfield's 1969 thesis that prudent government intervention in the capitalist economy can be very effective in inducing growth and smoothing out the market cycles that Marx thought would be the death of capitalism.

DAN A. COTHRAN

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The Origins of Socialist Thought in Japan. By John D. Crump. (New York: St. Martin's Press, 1983. Pp. 374. $30.00.)

Selective emulation of the West by Japan during the Meiji restoration inadvertently resulted in an unrestrained influx of a wide diversity of political philosophies, including socialism. Before 1905, Japanese socialist thought reflected external influences including Narodism from Russia, social democracy from Europe, Fabianism from Great Britain, and the Lib-Lab movement from New Zealand. From 1905 to 1918, socialist thought became much more radical, reflecting the influences of anarchism and syndicalism. Crump painstakingly delineates philosophical sources and subsequent failures of socialist thought to become an alternative to the emerging capitalism of the Japanese state. Socialist thought did not have sufficient appeal to become a sustained ideological alternative. Accordingly, the problems of widespread development and acceptance are attributed to the lack of fundamental points of reference with which the Japanese could easily identify. Also, the traditional classes of Japan did not constitute an adequate population base for a socialist movement. The samurai elite were at the forefront of the capitalist revolution. The peasantry, transients between rural and urban economies, did not provide stability in the urban centers or add to the growth of a working class. The artisan class, finding their skills redundant to industrialization, were contemptuous of the emerging common wage laborers. The merchants, in league with wealthier samurai, were beginning to be part of the ruling elite. Even the working class, consisting of contract laborers (mostly young women), could not easily identify with the socialist message and gradually became estranged from socialist methods and arguments.

Socialist thought and the socialist movement further suffered from the absence of indigenous traditions compatible to socialist idealism, narrow appeal, and misinterpretation. Socialist concepts were alien to the Japanese. For example, there was no literal translation of "bourgeoise" and "proletariat." These were translated as *shinshi batsu* (gentleman clique) and *heimin* (common people), certainly not revolutionary terminology. Loyalists to the socialist cause were few in number —estimates range from between 200 to 3,000, depending upon which source is consulted. Because of the varied external influences, socialist thought was often misinterpreted, misunderstood, and complicated by confusion and ineptitude on the part of its adherents. Further, the excesses of the capitalism that Marx described were simply not evident to the Japanese working classes. Instead of being an alternative *to* capitalism, socialist thought eventually became viewed as an alternative form *of* capitalism.

Crump's work is comprehensive, depending upon extensive Western and Japanese language source material including literature, letters, personal journals, interviews, and speeches. However, the rather diffuse, inchoate nature of socialist thought in Japan during the period covered creates an almost untenable basis for analysis. Consequently, there are several problems with the presentation. First, there is an overriding limitation imposed by Crump's very narrow definition of socialism. Socialism consists of two basic tenets: abolition of the money economy and the emergence of a "pure" socialist state. Such a narrow definition is totally inadequate as a framework to analyze the realities of Japanese society and the growth of socialism. Second, and perhaps most important, is Crump's failure to fully develop the influences of cultural traits of Japan that mitigated against the growth of socialism. Third, the study does not consider the influence of the appeal of other political philosophies, many of which had greater impact than did socialism. Fourth, the time frame is far too limiting. Ending with 1918, the impact of the Russian Revolution of 1917 is not considered but is instead left to "another story . . . another day and another volume" (p. 336). Fifth, there is too much evaluation of Western socialist thought, much of it extraneous to, and at the expense of, a more in-depth analysis of Japanese efforts and interpretations. Sixth, the conclusions about the impact of external socialist movements, activities, philosophers, and socialism on Japan is largely conjecture. This well-documented study certainly identifies the main sources, thinkers, and problems of socialism in early modern Japan. However, it simply does not present a systematic balanced analysis of the origins of socialist thought *within* Japan.

L. JEROLD ADAMS

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On Britain. By Ralf Dahrendorf. (Chicago: University of Chicago Press, 1982. Pp. 200. $6.95, paper.)

In this Ralf Dahrendorf's valedictory statement on the United Kingdom as he prepares to leave the London School of Economics (LSE) and return to German politics? It has that appearance, and the announcement in early 1983 that he would not accept a second 10-year term as LSE Director suggests an affirmative answer. Commissioned by the BBC to accompany a program series that apparently was never broadcast, *On Britain* was issued on its own in 1982. In Dahrendorf's words, "this is a book of critical affection" (p. 16) beginning with reflections on his first visit to the U.K. in 1948; continuing with an overview of the current state of British society, economic life and politics; and offering suggestions for reform throughout.

Dahrendorf, the author of *Class and Class Conflict in Industrial Society* (Stanford University Press, 1959) and other books, is a leading social theorist of the conflict school, a sometime German political figure and commissioner in the European Economic Community, and an important international academic leader in his position at the LSE. We have the right to expect much.

On Britain is written for the educated BBC listener. It is not a polemic. Reflecting Dahrendorf's dialectical approach to social theory, the book catalogs the strengths and weaknesses of contemporary Britain. The strengths of British society are its continuity and tradition, intellectual and scientific excellence, liberty and social solidarity (pp. 24-29). But these strengths also contribute to the U.K.'s current weakness and the tendency to view social, economic, and political conflict as a zero-sum (p. 13), or even a negative-sum game (p. 116). This approach is seen in race relations (pp. 35, 67-72), in industrial relations (pp. 83-86), and in the system of adversary politics (pp. 87-92).

Echoing Martin Weiner's *English Culture and the Decline of the Industrial Spirit* (Cambridge University Press, 1981) and earlier, A. V. Dicey, Dahrendorf sees another problem in the failure of Britain to continue to support the value system appropriate to an industrial society. "Today, Britain is that strange paradox, a non-industrial industrial society" (p. 44). It is not the class system that is at the root of this problem. Indeed, the decay of the traditional class system has left an ever larger part of the population without secure status and the will to work or earn their way in society (pp. 52-54).

What can be done to preserve the civil society that Dahrendorf and others have so admired over the years? As befitting a BBC publication, the prescriptions are not extreme and seem aimed mainly at the symptoms rather than the underlying value system. Some of his reforms include the single transferable vote (STV) system of proportional representation that would weaken the practice of adversary politics (pp. 116-118) and help the Liberal-SDP Alliance, but that cannot ensure that the system will ultimately operate differently. The development of a pattern of industrial democracy based on German codetermination would require labor and management to meet together (pp. 84-86) but cannot ensure that lasting cooperation will result. The short-term approach to politics might be changed by the development of a think tank modeled after the Brookings Institution where "those who take time to think and those who are involved in decisions meet and inspire each other" (p. 97), but that does not ensure that correct decisions will be made. A formal Bill of Rights might strengthen the tradition of liberty (pp. 123-125), but by Dahrendorf's own admission, civil liberties need more than paper guarantees (p. 25).

None of these reforms deal with the underlying question of how to modify the British value system to restore emphasis on competition and individualism, the very values that Dicey saw eclipsed in the nineteenth century. It is unfortunate that Dahrendorf does not offer advice on this. The efforts of the Thatcher government to reduce the size of the public sector through sale of various state-owned industries and to tolerate levels of unemployment unprecedented since the 1930s show that others are less reticent on the issue.

On Britain is a modest book. It does have some interesting insights. For example, Dahrendorf sees appeasement in the 1930s as an attempt by Britain to prevent the United States from replacing it as a major world power (p. 141). The book is marred by some editorial lapses. Harold Rose (p. 98) is actually Richard Rose (p. 195, chap. 13, n. 1). There is a certain innocence in Dahrendorf's view of the spontaneous nature of Parliamentary debates with the Speaker having no guidance on who to recognize (p. 87). Specialists will prefer Samuel Beer's *Britain against Itself: The Political Contradictions of Collectivism* (Norton, 1982), but students of comparative politics in general will benefit from Dahrendorf's European perspective on the crisis facing one industrial society in the twentieth century.

WILLIAM D. MULLER

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British Trade Unionism against the Trades Union Congress. By Gerald A. Dorfman. (Stanford, Calif.: Hoover Institution Press, 1983. Pp. vii + 165. $24.95.)

During the 1979 British strike wave—the "winter of discontent"—which resulted in the eventual fall of the last Labour government, BBC interviewers often asked people on the street what they felt was the cause of the labor strife. One union member boldly stated that "the unions have too much power." When the interviewer noted the seeming contradiction, the rank-and-file worker retorted, "Oh, not *my* union, it's those *other* unions."

This theme of a union movement divided against itself underpins Gerald A. Dorfman's *British Trade Unionism against the Trade Union Congress.* Dorfman argues that the Trades Union Congress (TUC) "suffers from its construction as a loose federation" (p. 3) of numerous constituent members. The unions have been essentially unwilling to commit their own institutional resources to the construction of an organizationally strong TUC. Paradoxically, this reluctance coexists with the various unions' repeated urgings during this century that the TUC press the government (particularly Labour governments) for full employment and adequate working standards. The book essentially holds that the TUC "has constantly been unequal to the collectivist relationship it encouraged" (p. 3).

Dorfman locates the cause of this TUC weakness in the failed General Strike of 1926, noting that "its conduct gave a black eye to the concept of powerful TUC leadership" (p. 9). Consequently, when leaders such as Walter Citrine and Ernest Bevin later argued for more power for the TUC, as well as for consultation on economic and industrial policy, they were far more successful in attaining the latter than the former—as the social and economic contract of 1944 showed.

The establishment of a pattern after World War II of a legitimized union presence without active interunion coordination, Dorfman asserts, resulted in the growing negative influence of the TUC. Specifically, it meant the TUC always possessed the power to veto distasteful government policies yet never developed sufficient positive influence to adopt creative alternatives. But as long as the British economy could withstand the postwar union emphasis on fragmented wage bargaining, the TUC and the unions were not forced to confront their fundamental lack of cohesion.

The book descriptively shows how the economic and political changes of the 1970s and early 1980s drastically underscored the limitations for the labor movement of a fragmented organizational structure. The divisions among the Callaghan government, the TUC, and the unions soon ushered in the explicitly antilabor Thatcher regime, with disastrous implications for the entire labor movement.

Dorfman's solution to the labor movement's impasse is for the unions and the TUC to develop "a much closer *intra-union* 'political' relationship" (p. 131). Specifically, he argues that if union leaders were also to serve as full-time TUC officials, they would better understand the "business of national trade unionism" (p. 132). *British Trade Unionism against the Trades Union Congress* is clearly helpful in highlighting the contemporary demise of the British labor movement's power, yet it is much less successful in dealing with its causes. The emphasis on the 1926 General Strike does not address why there are so many unions in the TUC and why their interests have been so parochial. Students of European industrial relations would be wise to supplement their reading of Dorfman's short book with an examination of E. P. Thompson's *The Making of the English Working Class* (Vintage, 1970), Ronald Dore's *British Factory-Japanese Factory* (University of California Press, 1973), or Ian Maitland's *The Causes of Industrial Disorder* (Routledge & Kegan Paul, 1983). Each of these works thoroughly analyzes how patterns of industrial production dating from the nineteenth century played a major role in the establishment of a fragmented, craft-based union structure that emphasized aggressive wage bargaining but much less of a comprehensive vision. Dorfman's recommendations for closer union and TUC cooperation certainly are well-taken. Yet for the labor movement to rebound from its recent setbacks, it must couple changes in its leadership structure with a much more comprehensive understanding of deeply entrenched structural characteristics. In doing so one could then more clearly analyze organized labor's possible role in shaping the continuing evolution of the British economy.

CHRISTOPHER S. ALLEN

Harvard University

The First Yugoslavia: Search for a Viable Political System. By Alex N. Dragnich. (Stanford, Calif.: Hoover Institution Press, 1983. Pp. xii + 175. $24.95.)

Despite his intention of filling a need for "an objective general survey of Yugoslavia's search for a viable political system in the period between the two world wars," Dragnich has nonetheless written a book whose bold but casually phrased statements are likely to arouse polemics among those concerned with that country, in both its past and present incarnations, and on the basis of their academic and their political interests.

In a way, *The First Yugoslavia* is a "revisionist" history, combating the often-repeated stance that the Kingdom of the Serbs, Croats, and Slovenes (later Yugoslavia) failed because of Serbian hegemony, intransigence, and narrow-mindedness. Nonetheless, Dragnich accepts that failure as almost inevitable because of the fact that "attendant problems" (p. 57)—largely Serb-Croat discord and the lack of a unifying consensus concerning the nature of the state itself—were too overwhelming to be expressed and handled through nascent and "unfamiliar democratic institutions" (p. 57). The timing of, and the times for, nation-building were inauspicious; the first Yugoslavia was founded under wartime conditions, "hurriedly and without adequate preparation" (p. 146). Those Croats responsible for entering into the union (members of the National Council) were not to be those (the Croatian People's Peasant party) responsible for carrying it out. The Serbs had had democratic, parliamentary experience in governing a homogenous, sovereign nation (the earlier Kingdom); they accepted the new state as "a continuation of an already existing unity" (p. 58). The Croats, previously a minority in the multinational Austro-Hungarian Empire, continued to oppose the state, now viewed as a force for Serb domination, through passive and/or actively obstructive tactics. The Serbs, Dragnich suggests, were "naive" in believing that through time and a democratic dialogue the Croatian masses could be won over to a Yugoslav identification. Instead, as chaotic parliamentarism was replaced first by open dictatorship and then by "guided democracy," the Serbs fought for "the restoration of democracy (p. 98) while in Croatia, where parties had formed along nationality rather than programmatic lines, the opposition remained "separatist and secessionist" (p. 149). Meanwhile, the Serbs, beset by factionalism and proliferating parties, "did not have a determined Serbian political line on the question of their relations with the Croats" (p. 149). Thus, as the approaching war ended all efforts at constitutional revision, and the first Yugoslavia itself, there was no "Serbian" counterpart to the Croatian *banovina* created by the *Sporazum* (Agreement) of 1939.

Dragnich attempts throughout the book to maintain an evenhanded stance towards Belgrade and Zagreb and to present each side's point of view. At one point he likens the Serbs and Croats to "two arrows going in opposite directions, passing each other in the night. There appeared to be a tragic lack of contact, a failure of communication" (p. 59). However, although Dragnich understands Croatia psychologically, he is not equally strong in doing so politically. It perhaps should not be so surprising that the Croatian leadership, if only for the sake of political survival and constituency appeal, eschewed reliance on "numerical majorities" (p. 148) in Belgrade in favor of structural guarantees of regional autonomy given the presence of phenomena that Dragnich himself mentions: the Croat's minority status, a unitary and centralized state apparatus, the appearance of the Kingdom as a successor state to Serbia, the omnipotence of intrigue in high ruling circles, the unruliness of parliaments, the use of incarceration as a means of exerting political pressure, inescapable arbitrariness at local administrative levels, and the possibility that, in a largely peasant and underdeveloped country, commitment to a Yugoslav identity was nowhere very deep. After all, as Dragnich also notes, the Serbs were also loath to agree to any form of federation leaving their people dispersed and vulnerable in non-Serbian administrative units.

The book would have been more useful to political scientists had Dragnich approached the first Yugoslavia—as his subtitle suggests—as a case study in nation-building and political development in an era when both phenomena were not in vogue. But this is most likely not the book he intended to write. Unfortunately, the survey form he actually chooses is too short to substantiate the controversial points he makes about equally controversial events and personalities; a chapter attempting to disprove Serbian exploitation and domination of Croatia is too brief to be convincing to those not already emotionally involved.

The first Yugoslavia, Dragnich implies in his final paragraph, does not compare badly with its successor because of its "respect for human rights and the integrity of the judicial process" (p. 152). After reading the book, one is left with the dominant impression of how little the Yugoslav political agenda has changed across the last six decades, and how fragile are consenses reached in a state in which each communal group is inclined to pursue its "national" interests and perspectives, if need be at the price of political tranquility for the whole.

KAREN ROSENBLUM-KALE

University of Southern California

The Politics of Canadian Airport Development: Lessons for Federalism. By Elliot J. Feldman and Jerome Milch. (Durham, N.C.: Duke University Press, 1983. Pp. xix + 261. $28.50.)

Since World War II, the Canadian federal government has undertaken a series of ambitious projects designed to place Canada at the forefront of world aviation. Beginning with the $400 million Avro Arrow supersonic fighter program that was begun in 1945 and abandoned in 1959 and continuing through the present faltering Canadair Challenger executive jet project, these efforts have been largely unsuccessful. Airport development is no exception, as this book carefully documents.

One of the Trudeau administration's most expensive and complex proposals in the late 1960s and early 1970s was the establishment of aviation infrastructure through the development of air facilities in Montreal, Toronto, and Vancouver. The world's largest airport was to be built outside Montreal as a "gateway to Canada" that would relieve future burdens at the existing Dorval airport and serve as the prototype of a new generation of super jetports. It was widely acclaimed in the halcyon days of expanding air travel and economic growth, even by some local residents whose land would be appropriated. Complementary facilities would be constructed at Toronto and Vancouver. Feldman and Milch describe the failures in all three locations: the completed but underutilized Mirabel airport outside Montreal, the acquired but unbuilt site for a second Toronto facility, and the abandoned project in Vancouver for a second major runway.

Feldman and Milch conducted more than 120 interviews between July, 1975 and November, 1977 in Ottawa, Montreal, Toronto, and Vancouver with those directly involved in the projects. Their methodology is sound, and their research thorough.

Less than one-half of the book is devoted to the three cases, and the balance to airport planning in relation to the national agenda and challenges to Canadian federalism. There is some repetition of details from the cases. There is no discussion of the patriation controversy and the 1982 reform of the Canadian Constitution, nor is Quebecois sentiment and Anglophone-Francophone antagonism accorded great importance. The "lessons" of the subtitle are concerned with the relation of state and citizen (expropriation of property, participation, protest), rationality in government, intergovernmental relations, and issues of national unity.

There is much of interest in this book to those intrigued by crossjurisdictional solutions to intergovernmental problems. For example, Feldman and Milch clearly show the folly of adopting different federal strategies for co-equal provinces. Ontario provincial officials were eagerly courted by the Ottawa Ministry of Transport, while those in Quebec were ignored; land acquisition for the Mirabel project (88,000 acres) proceeded differently than at the Pickering site northeast of Toronto and involved offers on a per/acre basis of approximately one-tenth those made to Ontario owners, a fact that—when discovered—generated understandable protest in Quebec.

The image that emerges from this book is one of political expediency and bureaucratic bungling. Airport development provides a good illustration of the difficulties of treating provinces equitably in the Canadian federal system—and, by extrapolation, in others—and the problems of planning and executing unique and complex projects.

KEITH M. HENDERSON

State University College at Buffalo

Steering the Polity: Communication and Politics in Israel. By Itzhak Galnoor. (Beverly Hills, Calif.: Sage Publications, 1982. Pp. xii + 411. $25.00.)

Nearly every graduate student in political science in the 1960s read Karl Deutsch's *Nerves of Government: Models of Political Communication and Control* (Free Press, 1963). It is more than likely that Itzhak Galnoor was one of them. His own work, *Steering the Polity,* is a useful and original application of a theory of political communication to the political system that Galnoor is most familiar with, Israel. Even more than Deutsch's model, though, the present work is primarily concerned not only with a case study, Israel during its first two decades of statehood, but also with the crucial importance of political communication in a democracy.

Galnoor makes a presentable case for communication theory in chapter 1. In modern societies, particularly democracies, how much a citizenry knows (or doesn't know) can be critical. Not without reason there is a necessary distinction between political development and democratic political development, a contrast that is often neglected by comparativists. Obviously, communication in democracies is less inhibited and more viable than in nondemocratic systems. Communication in full swing, for example as in election campaigns, takes off in the sense of new channels being opened (p. 25). The more communication there is, the greater democracy's chances. And, "the more persons participating in multidirectional transmission of political information, the more they become 'political communicators' and the higher the probability of further democratic development" (p. 25).

So far, so good. Actually, Galnoor's observation that there are differences to be noted between political development and democratic political development is an intriguing one. Hopefully, it will be the germ of a later study.

The actual focus of the study is upon Israel's early development into a mature polity. Israel as a case study is especially interesting because it is one of the few examples of recent political development that has been essentially democratic. Galnoor wisely allows for the obvious but often overlooked fact that each political society is "unique" and therefore may not completely fit every niche in the theoretical framework.

There is also a careful distinction to be made between having access to, and participation in, politics. Unfortunately, access does not guarantee participation. Even so, ideology does not have to get in the way of democracy. As Galnoor points out, "Zionism as an ideology embraced democracy, not only as a fundamental argumentation but also on the operative level" (p. 302). Ideology was not incompatible in Israel primarily because not all of the political parties interpreted Zionism in quite the same way and because a few actually rejected Zionism. This it would seem is an important lesson in political development: Ideology does not have to be undemocratic (or even antideemocratic as it is in many developing societies) if it is more or less shared by divergent groups, as long as these groups have equal access to the political process.

Galnoor's treatment does not neglect what he refers to as "peripheral groups" (pp. 348-366). Of particular interest, of course, is the Israeli Arab minority. There is ample justification for the conclusion that Zionism as an ideology contains "democratic elements" (p. 359), if only because the Arab Israelis participate to the fullest extent possible.

It is difficult to disagree with the implications Galnoor derives from his incisive study. It is certainly not an overstatement to suggest that "political development starts with the appearance of a political communication network" (p. 375). Where communication has an arrested network or blocked channels we cannot expect development that is democratic. Understanding this essential condition is as important for comparativists and is as revealing of political development as the more easily identifiable economic and social indexes.

MARTIN SLANN

Clemson University

On Cultural Freedom: An Exploration of Public Life in Poland and America. By Jeffrey C. Goldfarb. (Chicago: University of Chicago Press, 1982. Pp. x + 173. $25.00, cloth; $12.50, paper.)

This book was completed before the Polish regime cracked down on Solidarity and imprisoned Lech Walesa. Thus, Goldfarb can refer to Poland, "with its advanced development of cultural freedom despite significant repressive obstacles" (p. 1). He rejects the views of "recent converts to anticommunism" (p. ix) who believe that the repression of Solidarity and the Polish renewal constitute "proof of the impossibility of change in the 'communist world' " (p. ix). Goldfarb feels that constraints on cultural freedom in

Poland are political, whereas such constraints in the United States are economic, although he views "McCarthyism" as a threat to such freedom.

"By situating ourselves between East and West," writes Goldfarb, "we can appreciate the invisible nature of constraint in the West, as well as the less than apparent supports of cultural freedom in socialist societies. We often overlook systemic constraints faced by artists, intellectuals, and scholars in such societies as America, and even more often fail to recognize the powerful social forces which support freedom in societies such as Poland" (p. 5). In chapter 6, in a section titled "Anticommunism as a Repressive Ideology," Goldfarb writes that "with world hegemony and political repression of critics of capitalism at home, American capitalism reached its zenith" (p. 105).

Goldfarb rejects the "totalitarian-pluralistic continuum" used by Paul Hollander in his comparison of Soviet and American societies. Goldfarb's approach "has the advantage of permitting one to reach conclusions on the basis of observation, reason, and judgement," thus preparing the reader for "the unexpected, the ironic, and the inconvenient" (p. 151). Formal constitutionally guaranteed freedoms are "neither a necessary nor a sufficient support for the realization of cultural freedom," whereas "comparatively free cultural life" has "flourished at times without such guarantees in the East" (p. 151).

Goldfarb has "drawn upon the thoughts" of Max Horkheimer and Theodor Adorno, Walter Benjamin, Herbert Marcuse, Jurgen Habermas, Barrington Moore, Jr., Isaiah Berlin, Victor Zazlavsky, Stanislaw Ossowski, Jan Strzelecki, Zygmunt Bauman, Dwight McDonald, and Edward Shils, as well as the works of Karl Marx, Max Weber, Emile Durkheim, and Georg Simmel. He praises Hannah Arendt's "optimistic reflections on the 1956 Hungarian uprising, seeing in it the possibilities for free cultural and political action. In Poland such possibilities have been partially realized, promising even more" (p. 153).

In his conclusion, Goldfarb rejects the notion of free versus unfree societies, because "to posit freedom as a static state is to avoid inquiry into its bases, limits, and potential" (p. 153). The Reagan administration, he charges, has turned away from "nonevaluative pluralism" toward "nostalgic elitism," and "universities are facing demands for practical results and are under attack by the new administration in the form of major cutbacks in financial support for students" (p. 154). These changes, he writes, "point in the direction of greater market determination of cultural life" (p. 154). Goldfarb admits that Solidarity has now experienced "massive repression," but insists that his theories about cultural freedom in Poland have not been invalidated.

ANTHONY TRAWICK BOUSCAREN

Le Moyne College

Revolution in Iran: The Politics of Countermobilization. By Jerrold D. Green. (New York: Praeger Publications, 1982. Pp. xiii + 199. $24.95, cloth; $4.95, paper.)

Green has provided an interesting and novel conceptual framework for the analysis of the Iranian Revolution. Although Green is cautious about the applicability of his prototheory elsewhere, the overthrow of Diaz in Mexico might be an interesting case to test his concepts. Still, so many things seem to have been unique to the Iranian Revolution that generalizing beyond this particular case seems unwarranted. What makes Green's argument so persuasive is that he was there. Almost reminiscent of John Reed's *Ten Days that Shook the World* (Random House, 1960), Green gives the reader an account of the Iranian Revolution from the grass-roots level. He was not a diplomat or a well-established scholar with access to the highest levels of government. Rather, Green was the observer, in sympathy with the opposition but not biased in his scholarship.

Green first outlines the Shah's efforts to socially mobilize the Iranian population behind his Shah-People Revolution (initially called the White Revolution). He stresses (perhaps too much) the role of the mass media in trying to alter Iranian lifestyles along lines desired by the Shah and his supporters. The effect was counterproductive contends Green, leading many Iranians to charge the Shah with eroding Iranian culture by a program of "Westoxification." The consequences of the Shah's development ethos demanded greater real political participation by the emerging urban classes. Instead the Shah offered Iranians "pseudo-participation," that is, institutions of no substance. As Green correctly notes (p. 72), "had political participation kept abreast with this social mobilization, political stability might have been preserved."

The failure of the Shah to satisfy calls for real political input led to a "crisis of participation" in Iran. The government could either allow for more popular participation or increase its levels of repression. In the case of Iran, both strategies were tried, neither successfully, always too late, and never to the degree necessary for effectiveness. This allowed those opposing the Shah's rule to countermobilize. Green identifies the steps that led to such countermobilization in Iran: The Shah was both unwilling and unable to employ coercive powers sufficient to stem any popular uprising; political demands were simplified as both pro-Shah and anti-Shah forces reduced their definition of the situation to the lowest common denominator; both sides became "polarized," with each denying the legitimacy of the other; traditionally nonpolitical sectors of society were brought into the conflict; and each pole actively sought popular support.

Green traces the events immediately preceding the Shah's fall in detail, sharing with the reader some exciting vignettes of his own experiences at the time. Surprisingly, violence by those counter-mobilized was kept to a minimum. Popular demonstrations, strikes, funeral processions, and public marches took the place of the barricade. Meanwhile, the Shah was doing everything possible to make compromise with the opposition impossible, either by increasing the use of coercive power or by suddenly offering an appeasing reform too late. Green also notes the ambiguity that surrounded the goals of the Revolution beyond that of bringing down the Shah. Khomeini's popularity was undisputed, but few envisaged the Iran to come. We still await a solid study of the fall of the Pahlavi dynasty. Green's book is a step in that direction.

The book also includes a useful chronology of the Iranian Revolution as well as an extensive bibliography that should prove valuable to undergraduate students.

RICHARD H. PFAFF

University of Colorado

Soldiers without Politics: Blacks in the South African Armed Forces. By Kenneth W. Grundy. (Berkeley: University of California Press, 1983. Pp. xiv + 297. $24.50.)

The title of this book has been exerpted from Mozambican President Samora Machel's telling comment that "A soldier without politics is an assassin" (p. x). Kenneth Grundy argues that black soldiers serving South Africa merit criticism, although he readily acknowledges the compelling economic pressures that induce a small but growing segment of the black population to enlist in the military and police apparatus of the white supremacist regime. Grundy does not accept the optimistic view that the recruitment of Africans, Indians, and Coloureds constitutes a meaningful step toward deracialization, where progressive military technocrats initiate and legitimize a process of reform ultimately spreading from the barracks to the society at large. He argues that the utilization of black soldiers simply provides an additional mechanism for the white powerholders to manipulate "racial and ethnic differences to maintain the political and economic regime" and "to tamp down civil unrest and to control and reverse the military threats on the borders" (p. 278). In his view, black South Africans "lack the vital perception of interest . . . that for them the system is worth defending" (p. 280). Hence black soldiers are at best an impressed force or at worst mercenaries employing violence for "short term, selfish motives" (p. x) instead of some principled political cause.

Soldiers without Politics is a pioneering work that ferrets out a remarkable amount of information about the evolution and present status of South African military structures, objectives, attitudes, and roles in society and government. A less zealous researcher might have been intimidated by the formidable problems that South African security restrictions pose for data collection on even routine military and internal security matters. Drawing upon official sources, skillful interviewing, and shrewd observation, Grundy has probed deeply into the still more sensitive issues of arming, deploying, and utilizing black soldiers and police. In successive chapters, he discusses the history, structure, training, force levels, remuneration, treatment, duties, and impact of black police, Coloured and Indian soldiers and sailors, African units, homeland armies, and indigenous Namibian forces. Orienting his analysis around these different ethnic components has led to some repetitiveness, in part due to problems of integrating chapters which had previously been published as separate articles. However, Grundy's approach also reflects the extent to which official South African thought and action sustain fragmented approaches to the different black groups and reinforce established caste relationships and patterns of segmentation.

To comprehend the contemporary position of blacks in the South African armed services, Grundy contrasts the actual roles of black forces in South Africa's wars with the persistent but erroneous view that blacks were traditionally excluded from the military and especially from combat. He portrays the evolution of black military participation as a piecemeal process demonstrating the capacity of the South African regime for flexibility, reform, and even creativity in the means selected for perpetuating the "white dominated capitalist order" (p. 274). In the process, Grundy succeeds in integrating his study of the armed forces and police with the already voluminous literature on apartheid policies, security legislation, economic exploitation, and Afrikaner nationalist ideology. His conclusions on the militarization of South African society and the prospects for military intervention in politics must be read in conjunction with his superb monograph that stripped away the veil of anonymity from the state security apparatus and created major shock waves in official circles (*The Rise of the South African Security Establishment,* Bradlow Series, No. 1, August 1983, Braamfontein: The South African Institute of International Affairs).

The faults of the book are relatively minor. In concluding, Grundy might have returned more explicitly to the paradigms that he discussed in his introduction regarding the role of the military in ethnically polarized societies. He could also have considered whether the South African armed forces have any external linkages with Western or Israeli military and intelligence agencies which might mold or alter the roles assigned to black units. But the most basic problem is beyond his control. The black soldier has not spoken for himself on his roles, nor is he likely to do so in a situation where he is used to preserve exploitative patterns and even "to participate in the disruption of the affairs of neighboring peoples" (p. 278). However, the descriptions that Grundy has provided of black South African forces speak eloquently to their marginality, exploitation, and alienation from fellow blacks. *Soldiers without Politics* is essential reading for all students of Southern Africa and scholars interested in the political dimensions of military and security organizations.

RICHARD F. WEISFELDER

The University of Toledo

Change in South Africa: Blind Alleys or New Directions? By Christopher R. Hill. (London: Rex Collings, 1983. Pp. ix + 224. L12.50.)

As a former member of the British diplomatic service and faculty member of the then University of Rhodesia and the current Director of the Center for Southern African Studies at the University of York, Christopher R. Hill is well equipped by both experience and his education at Cambridge University to meld observation with theoretical concerns. He also has normative commitments through his position as an overseas member of the highly regarded Council of the South African Institute of Race Relations. What Hill has provided is a remarkable blend of the view from the outside and from the inside made possible by a sabbatical leave spent at the Afrikaans-medium University of Stellenbosch at the Cape—the Alma Mater of those South African Prime Ministers who obtained a university degree.

What is so attractive about this scholarly volume, which is amply documented and indexed and includes a selective bibliography, is Hill's dispassionate analysis of the contours of the Afrikaner political elite, especially university faculty members who are in the various social science disciplines. Here he explores the nature of the prestige hierarchy of Afrikaner academics and the links established between the National party government of Prime Minister P. W. Botha and the academic insiders. Much of the antecedent literature concentrated on the *Broederbond,* a secret group of Afrikaner men in many walks of life who were determined to have their prescriptions for Afrikanerdom implemented by the National party government. Hill's concerns are broader than the *Broederbond* and have greater comparative utility because they focus upon a more openly accessible elite—the university faculty member. While at Stellenbosch, Hill was able to observe the ebb and flow of university student politics which, remarkably enough, were followed carefully by the prime minister and his cabinet colleagues, because Stellenbosch students were indeed candidate members of the political elite. In contemporary South African politics, the Orwellian view that Afrikaner students are more equal than English-speaking ones suggests why Hill focused especially on the University of Stellenbosch. Those Afrikaners in the Cape have had a reputation of being more *verlig* (moderately conservative) than their kinsmen in the other three provinces.

In examining the rate and direction of change in South Africa, Hill concentrates heavily on the Afrikaners, and especially upon the *verligte* elements in Afrikanerdom, who are bitterly opposed by the *verkramptes,* or narrow-minded Afrikaners who cherish all the perquisites granted them by apartheid within what is basically a zero-sum society devoted to maximizing white values in an authoritative, if not authoritarian, manner. Hill is noticeably adroit in illuminating and evaluating the welter of theories, often generated by South African exiles resident in Britain, explaining change or continual domination in terms of class, race, and ethnicity. He skillfully demonstrates what each particular school of thought (clustered around a particular periodical, a British university department or unit, or individual) contributes to clear-headed thought about the past, present, and future of South Africa.

Hill places primary emphasis upon the Afrikaner *verligtes* as agents of change, and he provides a careful inventory of change in the industrial workplace that has long been the battleground between *verkrampte* white trade unions and cost-conscious English-speaking business managers, with the African workforce used as pawns. Moreover, he discusses the array of federal and confederal schemes that South African whites have generated within the consociational approach to intrasocietal conflict, which leads him to examine very briefly regional and extraregional pressures on South Africa. This lucid, thoughtful, and gracefully written book will enrich both comparative political science and the quality of the political debate over South Africa.

RICHARD DALE

Southern Illinois University at Carbondale

Policy Dynamics. By Brian W. Hogwood and B. Guy Peters. (New York: St. Martin's Press, 1983. Pp. x + 289. $27.50.)

Policymaking in industrial democracies has been going through a "time of troubles" in the decade since the Oil Shock. Problems such as inflation, recession, fiscal crisis, tax revolt, decline in public confidence, and cutbacks in public services have become the staples of journalism as well as a good bit of the substantive policy studies literature. *Policy Dynamics* represents one of the first major analytical efforts to go beneath the surface of events, even beneath such trendy topics as "system overload" and "cutback management," to investigate the impact of recent experience on the fundamental concepts and models we use to organize our knowledge of the policy process. It is an important book that adds significantly to our conceptual equipment for understanding how policy change works.

Hogwood and Peters begin with the fairly simple insight that because of the "crowding of the policy space" after 30 years of welfare state growth and the newly felt resource constraints on policymaking, most policy change in industrial democracies will be "policy succession"—the replacement of an existing policy, program, or organization by another—rather than the initiation of totally new policies. Other writers might slide by this idea in a page or two. Hogwood and Peters turn it into a major book by subjecting the original notion to a rigorous and methodical examination of all its intellectual and practical implications. Their approach constitutes a veritable *tour de force* of conceptual explication and development.

The book's focus is on change and process rather than on structure and substance. It starts from the model of the policy cycle that is more or less standard in the literature: agenda-setting, policy formulation, legitimation, organization, implementation, evaluation, and termination. Rather than simply tacking another stage labeled succession onto the cycle, Hogwood and Peters completely rework the model. They leave the basic stages nominally intact, but sharpen the conceptual basis of each by using their notion of policy succession to cast new light on each stage. Their strong point is suggesting new categories and new linkages. They develop several typologies of policy change that are useful improvements on the incrementalist literature. And the diagrams they use to illustrate these typologies are likely to find their way onto blackboards in many policy analysis courses as the book receives the wide readership it deserves.

The writing is rather dense. Sections, subsections, and even sub-subsections are numbered, headed, and often preceded by epigraphs. Undergraduates and persons who are not policy specialists may find the going somewhat slow. But Hogwood and Peters usually illustrate their points with well-chosen examples from American and British policy experience. These examples not only clarify the argument but also provide fascinating glimpses of contrast and comparison in the policy process of the two great English-speaking democracies.

Hogwood and Peters deal with a policy process model that for many of us had become a convenient but static set of pigeonholes for data. They have produced a much-improved set of propositions about how policy change takes place and some provocative suggestions about how to design policies to make it easier to replace them in the future. They believe that it is no longer possible "to boldly go" into the unexplored reaches of completely new policy areas. Policymakers will be more and more concerned with making improvements on the existing system. The book's approach reflects the same belief. Rather than blazing new trails outside the conventional framework, it explores what we previously thought of as known space. It shows us that creative thought can still pay big dividends in improving our understanding of those regions as well.

JAMES A. DUNN, JR.

Rutgers University-Camden

The Politics of Backwardness in Hungary 1825-1945. By Andrew C. Janos. (Princeton, N.J.: Princeton University Press, 1982. Pp. xxxvi + 370. $27.50, cloth; $12.50, paper.)

Political scientists have frequently looked to the past as a source of rich materials to test hypotheses purporting to explain social phenomena. Using a historical setting, Andrew C. Janos examines political change in Hungary, a country on the borderlands of Europe, through the prism of theories of development ranging from that of

Herbert Spencer to those of Barrington Moore and Immanuel Wallerstein.

Following the work of Moore, Samuel P. Huntington, and Reinhard Bendix, Janos suggests that the course of Hungary's development differed from that of the West owing to the country's level of political institutionalization, its cultural make-up, and the configuration of classes in its society. To this perspective, Janos adds that of Wallerstein, who stresses the impact of the "global socioeconomic system possessing its own logic, division of labor, and operational principles" (p. xviii). Combined, these two approaches lead Janos to question assumptions derived from three different theories of monolinear development associated respectively with the names of Herbert Spencer; Max Weber, Ferdinand Tonnies, Karl Mannheim, and Karl Polanyi; and Marx.

Janos shows that Hungary pursued its own distinct course of development for several reasons. Although Hungary possessed an advanced political system, it was backward economically. This backwardness led to primary emphasis on the state as the engine of modernization in the country. However, an etatist path entailed its own contradictions, which precluded Hungarian success in catching up with the West. Once the bureaucracy accorded a dominant role to itself, it sought to expand even further and therefore soaked up savings in the form of taxes to pay for its growth. The bureaucracy also siphoned off talent that might otherwise have undertaken entrepreneurial endeavors. Finally, state efforts to increase the literacy of the populace, while successful, provoked a feeling of "relative deprivation" on the part of all classes, who now compared their own circumstances unfavorably with those of their counterparts in the West.

These contradictions culminated in the rise of the authoritarian right in the interwar period. The advanced states of the European core continued to dominate the international economy, thus producing a desire among elites of weaker nations for the restructuring of the world system in their favor. The ideology of proletarian internationalism threatened the class position of Hungary's elites. Plebian elements who had been mobilized into power embraced right-wing political ideology that promised a revolution in the international system and repression of the lower classes at home. Antisemitism justified the expropriation of property held by the Jewish minority, whose wealth could be used in the quest for modernization.

Janos attempts many things in this volume, some of which are contradictory. Hungary cannot supply a "generalized concept of peripheral politics" because, unlike many nations on the periphery of the world system today, it "possessed a distinctly occidental religious and institutional heritage" (p. xxi). Moreover, Janos himself admits that "the rise of a bi-polar international system with new rules of the game" presents "a different analytic problem for the student of political change" (p. xxiii).

One must also question some of Janos's substantive conclusions. Intellectuals in Hungary experienced alienation in the early twentieth century owing to far more than the inability of the general populace to appreciate the artistic creations of the avant-garde (p. 171). More profound sources of alienation lay in the humanist intelligentsia's resentment at its subjection to the forces of the market economy, and in the Hapsburg Empire's policies of censorship and use of police spies, both of which involved curbs on freedom of expression. In addition, peripheral elites were not the first to reject the "liberal idea of restraint that the Occident has brought into the modern world" (p. 169). One need only recall that the West gave birth to Social Darwinism, a philosophy that erected survival of the fittest into a moral standard.

Janos draws on sources in five languages to produce a very readable work. The book contains insights into such diverse subjects as the reflection of Hungarian sociopolitical concerns in the literature of the period, the position of Jews in Hungarian society, and the influence of international public opinion on political practices in peripheral societies. *The Politics of Backwardness in Hungary* will be a source of intellectual stimulation and excitement for both students of the contemporary world economy and those of the Eastern European area.

STEPHEN R. BURANT

Shorewood, Wis.

Colombia: Portrait of Unity and Diversity. By Harvey F. Kline. (Boulder, Colo.: Westview Press, 1983. Pp. xv + 169. $18.00.)

Colombia is the third most populous country in Latin America, yet it is probably the most misunderstood country in that region. Too often it is understood in terms of what is going on in Mexico, Cuba, or the Southern Cone, but rarely is it understood on its own terms. To correct this, Harvey Kline provides an overview of Colombian society and politics. As the subtitle indicates, Kline's major theme is that there is no single, simple Colombian reality. Rather, there are competing and contradicting realities. Kline spends a significant part of the book detailing the complexity of Colombian society and explaining how it cannot be understood effectively with simplistic, foreign explanations. Unfortunately for the reader, Kline is able to explore only briefly the tremendous contextual richness of Colombian society.

This book is the fourth in Westview's "Nations of Contemporary Latin America" series. The book is organized into six substantive chapters and a conclusion that focuses on the future. The chapters deal with geography and demography, pre-1930's history, post-1930's history, government and politics, economic policy, and international politics. The strength of the book is its ability to deal with a very large topic with some coherence; the weakness is that important issues are raised and discussed only briefly, for example, the "dance of the millions" (the extensive penetration of foreign loans and investment in the early twentieth century). The discussion of *La Violencia* (the period of chronic and catastrophic rural violence from 1946 until the early 1960s) is discussed in only simplistic terms.

In other areas Kline demonstrates effectively the texture and complexity of Colombian society. He does an excellent job of this in explaining the development of the political party system in the country. Long dominated by two hostile and competing political parties, the Liberals and the Conservatives, Colombia reacted to the partisan violence of the 1940s through the 1960s by establishing a power-sharing system between the parties. Kline shows that the eventual effects of this power-sharing—called the National Front—have been to make the Liberal-Conservative division almost meaningless and to increase the fragmentation within the two parties.

The book raises a number of questions that are important for the future of Colombia: What will happen to the party system? (Will it become a competitive party system, or will intraparty splits make this competition meaningless?) What is the role of the Colombian military in the political system? (Will we see the military become politically active, as it is in the Southern Cone?) What is the nature of the country's economic policy, and how will domestic and foreign economic factors affect both the party system and the military?

Although the book breaks no new frontiers of research, it does summarize, update, and provide a unified understanding of a very complex subject. With the possible exception of R. Albert Berry et al.'s *Politics of Compromise* (Transaction Books, 1980), there has not been a comprehensive study of the Colombian political system since John Martz's *Colombia: A Contemporary Political Survey* (University of North Carolina Press, 1962). Kline's book closes that gap. One wishes only that it provided more detail to add to Kline's obvious understanding of Colombia.

BRIAN F. WALLACE

Capital University

Political Forces in Spain, Greece, and Portugal. By Beate Kohler. (Boston: Butterworth Scientific, 1982. Pp. 281. $59.95.)

My major objection to this book is its price, not its content. The first volume to appear in English comparing generally the politics of Spain, Greece, and Portugal, its primary contribution lies in its summary of these countries' current political forces. As such, its pricing warrants mentioning because it might preclude access by the majority of the clientele for whom the book would have primary appeal: students and the informed public. In contrast, country specialists will find little here that is not already general knowledge and available elsewhere.

The merits of the book are these. First, it offers an up-to-date survey of political developments in the three countries. The period covered spans the years dating from the demise of their dictatorships up through the consolidation of their new democratic regimes, as of December 1980. Kohler's second contribution is a systematic overview of political forces. In each instance the author gives a concise summary of the end of authoritarianism, the nature of the transition to democratic rule, and the current political order. In those instances where a clear-cut division of political forces into party groupings is not present, Kohler explains the nature of the social groups from which the parties are drawn and how identification of forces on the Right, the Center, and the Left can be used to develop a coherent framework for understanding shifting political alignments.

Third, this is the first book to deal effectively with the interaction between external and internal political and social forces and how the two combined have influenced political outcomes. The consolidation of democratic regimes in all three cannot be viewed apart from external interests in seeing that these countries' social, political, and economic forces be tied more effectively to their counterparts in Europe at large. The most important contribution here is the clarification of the role of West German foundations in influencing internal politics in all three countries according to a similar pattern. At the same time, Kohler makes quite apparent how distinct domestic contexts have led to very different outcomes in building these relationships.

Fourth, Kohler supplies the reader with excellent source material. This takes two forms: a bibliography listing major country sources and detailed endnotes after each section that provide not only specialized entries but also useful annotations with more relevant detailed information. Contained herein are sources frequently overlooked in North American scholarship—the most important of which are West German in origin. Converted into textual material, it is the richness and completeness of Kohler's sources that serve as the basis for the detailed country analysis.

This very detailed analysis of each country's political and social forces, which is one of the book's strenghts, is also the source of its difficulties. Although there is a logic for grouping together Spain, Greece, and Portugal—without consideration for Italy—the author actually engages in little comparative analysis. Kohler uses parallel categories to examine each case, but leaves comparison itself up to the reader. Just as there is scant internal cross-national comparison, there is also little in the Introduction and Conclusion bringing together common patterns and explaining why the decade of the 1970s, which is examined in such detail, was a momentous one for all three countries. Whereas Spain, Greece, and Portugal entered the decade on the periphery of Europe, by the end of the 1970s there could no longer be any doubt that their future, their options, and their constraints have become irrevocably linked with those of Europe as a whole. Integration into the European Economic Community will be the most difficult for Portugal, but its structural problems are not all that different from those faced by Spain and Greece. Certainly Spain has come the farthest in its internal transformations, but the point at which it began in the 1950s is not all that different. Such parallels as these argue for further comparative analysis of the three countries as cases central to any discussion of the interplay between nationalism and modernization in the contemporary world.

LAWRENCE S. GRAHAM

University of Texas at Austin

Conservative Politics in Western Europe. Edited by Zig Layton-Henry. (New York: St. Martin's Press, 1982. Pp. xi + 352. $27.50.)

Conservative Politics in Western Europe is a collection of 14 essays, all written by European scholars. The introductory essay, written by the volume's editor Zig Layton-Henry, is followed by essays on conservative politics and political behavior in Britain (Gillian Peele), Scotland (William Money), Sweden (Sten Berglund and Ulf Lindstrom), Denmark (Ib Faurby and Ole Kristensen), Norway (Bjarne Kristiansen and Lars Svasand), the Federal Republic of Germany (Alf Mintzel), Belgium (Anthony Mughan), the Netherlands (Hermann von der Dunk), France (Jean-Claude Colliard on the Giscardiens and Patrick Lecomte on Chirac's *Rassemblement*), Italy (Mario Caciagli), Spain (Kenneth Medhurst), and in what is the most interesting essay in the volume, Geoffrey Pridham deals with transnational conservative cooperation within the European Community and the European Parliament.

The volume makes a strong case for the resurgence of the conservative ideology across Western Europe, although the electoral failures of some conservative parties are also noted. As Layton-Henry comments, the conservative ideology has made a remarkable comeback since the end of World War II when the parties of the Right were discredited, rightly or wrongly, in most European countries due to the ties between conservatism and fascism and to the collaboration between conservative groups and the Nazis. But by the time of the early 1970s and continuing into the 1980s, the parties of the Right had regained their strength, even in such social democratic strongholds of Scandinavia.

But this swing to the Right can be overdrawn. There may have been a resurgence of conservatism in some specific countries in recent years, but when the European Community as a whole is analyzed by looking at the first (June, 1979) direct elections to the European Parliament, a different interpretation surfaces. Ronald Inglehart and Jacques-René Rabier, in "Europe Elects a Parliament," in Leon Hurwitz (ed.), *Contemporary Perspectives on European Integration* (Greenwood Press, 1980, pp. 27-51), observe that only in Great Britain, where the Labour party put on a dismal show in the 1979 elections, was there any ground to support a conservative resurgence. The swing to the Right across Europe was anything but evident if the composition of the 1979 to 1984 European Parliament is compared to that of the previous assembly, if the votes cast in the European Parliament are compared to that of the previous assembly, and if the votes cast in the European election are compared to those in the most recent national elections. The swing was very limited in a geographical sense (the United Kingdom).

As with any collective and collaborative work, some technical problems surface. The essays are of uneven quality, ranging from first-rate analyses (the essay on conservative party activists in Denmark by Faurby and Kristensen, for example) to what I interpret to be graduate student papers. Although not identified, several of the essays have been translated into English from the original, and some appear in that stilted academic English that has been described by the London *Times* (in an entirely different context) as "techno-English"—the language is correct but it does not make for easy reading. Some of the essays are overdocumented (one has some 64 notes in 19 pages of text), and it is difficult to follow the thread of the argument with so many side diversions. Finally, an index that contained some concepts and themes rather than just names of people and parties would have been more helpful. These criticisms, however, are really only cosmetic. The book, although not suited for the introductory student, serves as an important contribution for the specialist in the field of European party politics.

LEON HURWITZ

Cleveland State University

The Russian Intelligentsia: From Torment to Silence. By Vladimir Nahirny. (New Brunswick, N.J.: Transaction Books, 1983. Pp. 200. $22.95.)

Nahirny's objective is to increase our comprehension of the development of the Russian intelligentsia, particularly of the nineteenth century. His method is sociological in the tradition of Mannheim. Nahirny exposes the role playing and agonies of the "men of conviction" of Russian society with particular attention given to their social origins and to aspects of generational change. He avoids a simplistic class analysis by establishing a model of social differentiation appropriate to Russia of the time. Unfortunately, the reader must wait to the conclusion of the book to see the model comprehensively displayed. An earlier presentation of the model, together with clearer identification of time periods of his chosen examples of members of the intelligentsia, would have assisted the organization of the book. Despite these problems, Nahirny's work is a success. The ideologization of the intelligentsia is portrayed skillfully and with ample documentation from primary sources including fascinating quotations from diaries. At times Nahirny's penchant for quoting the words of the intelligentsia without translating their French or German will cause the less literate of us to tremble; but both the style of his writing and his analysis are convincing, comprehensible, and thorough.

What makes Nahirny so successful is his consistency in avoiding the substance and passion of the great debates of the Russian intelligentsia. Those have been written about and analyzed by students of literature, history, and politics for almost a century. Nahirny is making a distinctive contribution by methodically investigating and analyzing the "mode of orientation" toward ideas by the Russian intelligentsia. By this he means exploring hypotheses regarding both individual and group reactions to ideas and theories, particularly reactions that transformed ideas into "causes" and ideologies. Nahirny does not neglect the great issues of Russian intellectual debate, but rather uses those issues as the backdrop of his analysis. The variety of Russian responses to Western ideas is well elaborated, yet Nahirny moves consistently to a concluding theme that, despite this variety, a unified consequence emerges—ideologization of a total sort.

It is Nahirny's "mode of orientation" to this theme that will cause some readers to reflect on Western intellectual development instead of Russian. Nahirny appears to be restating some of the arguments about the "origins of totalitarianism," although with some distinctive convolutions. Lenin is portrayed as the culmination of the ideologization of the Russian intelligentsia in the crucial generation following the reforms of the 1860s. Nahirny gives those reforms immense importance, especially in terms of their consequence for urban life in Russia. The combination of those changes and the historical development of the intelligentsia provide the fertile ground for Leninism and terroristic ideology, according to Nahirny. Those who have difficulty with the totalitarian model of Soviet politics will have some misgivings regarding this aspect of the book. But the scholarship that produced the book will not be questioned, nor will be the judgment that this book will be regarded as one of the most important on Russian historical-political development of this decade.

PATRICK DRINAN

University of San Diego

Organized Democracy: Political Institutions in a Welfare State—The Case of Norway. By Johan P. Olsen. (Bergen: Universitetsforlaget, 1983. Pp. 1 + 246. $20.00, paper.)

The thesis of *Organized Democracy* is multifaceted. Olsen asserts first that "political institutions have modified in significant ways since 1945" (p. 7); second, that current analytical models have not kept up with these changes; and third, that our interpretation of these changes might reflect a theoretical misunderstanding rather than behavior that may have been viewed as pathological. Finally, what is needed, says Olsen, is a framework so that scholars might "better understand the role of organizational factors in political life" (p. 7). In essence, he has called for what he refers to as a "new institutionalism" that can focus on how, and to what degree, behavior and outcomes of that behavior are enrolled by political institutions.

Olsen employs a method that he calls "organization theory." He feels that it is ideal because it bridges the gap between the traditional and behavioral approaches and has the advantages of both approaches. The methodological goals of the book are relatively modest, so modest in fact that it might be asked whether anything can come from the inquiry. Olsen says that "no attempt is made to present a theory of organized democracy. But a few preliminary observations of political institutions in a contemporary welfare state indicate the potential value of organization theory to political analysis" (p. 10).

Olsen uses Norwegian government as the vehicle to develop his theoretical work. The book is divided into six chapters, presenting a fairly cohesive evaluation of the Norwegian case. Chapter 1 deals with the issue of the legitimacy crises hypothesis and in particular its application to Norwegian citizen initiatives. Chapter 2 examines the Storting (parliament) from a structural and policymaking perspective. Chapter 3 deals with the Norwegian cabinet and executive. Olsen feels that "the key questions here are the degree to which politically appointed leaders are able to give direction to the large administrative apparatus acting in their name and *how* they give this direction" (p. 78). Chapter 4 is an excellent characterization of the Norwegian civil service, being a prototype of the "Weberian ideal bureaucracy in action." Chapters 5 and 6 are attempts at capturing general theory with sweeping applications of organization theory. These chapters appear to be tacked on and do not quite fit the smooth flow of presentation found in the previous chapters. Olsen tries "to summarize the empirical studies on integrated organizational participation in government and identify the main theoretical themes that can be inferred from those studies and from organization theory" (p. 151).

One is left with the impression that too much has been attempted in a short space. The book is extremely well-documented, in fact, much of it reads like an annotated bibliography of everything and anything germane to the subject. The bibliography is excellent. If Olsen had continued with the case study analysis after chapter 4, or dealt with the exclusive task of developing a generalized organization theory and left off the case study, his task would have been simplified. As it is, the reader is left with segmented fragments that do not hang together well.

Due to the subject's ambiguity, it is difficult to say whether Olsen meets his objectives. It is also questionable how typical Norway is as a prototype for comparison. Norway is so replete with exceptions that little remains that is transferable as a general case to other democratic systems. However, on balance, *Organized Democracy* is interesting, conceptually provoking (albeit not cohesive), and does contribute to the growing scholarly literature on democracy in the welfare state, Norway.

JAMES L. WAITE

Central Missouri State University

Politics and Technology in the Soviet Union. By Bruce Parrott. (Cambridge, Mass.: MIT Press, 1983. Pp. x + 428. $45.00.)

Bruce Parrott's study of Soviet technological development is unique in its treatment of one of the most important issues in Soviet politics. Most previous studies on Soviet technology have been the domain of economists, who directed attention to the institutional framework of the Soviet research and development process. By contrast, Parrott is interested in technological development as a political issue subject to conflicting strategies and alternative interpretations and closely linked with the Soviet image of the outside world.

The major thesis of *Politics and Technology* is that throughout Soviet history technological policies have been determined by the interplay of two conflicting tendencies of thought within the Soviet leadership and elite. The "traditionalist" view, seeing capitalism as highly aggressive yet in the throes of an economic crisis, is characterized by an emphasis on indigenous technological development (in particular, military technology) and establishing technological autarky. The "non-traditionalist" view has portrayed capitalism as less aggressive and more dynamic economically than the "traditionalists" and has favored extensive technological ties with the West as a way of alleviating economic problems at home. Utilizing an astounding array of Soviet and Western sources, Parrott traces these two tendencies in the policy debates of the Kremlin from Stalin to Brezhnev, showing that at nearly every point in Soviet history considerable conflict has occurred over the choice of an appropriate technological strategy.

Parrott's careful and nuanced reading of the statements of a wide variety of Soviet leaders, as well as his attempts to establish connections between the context of Soviet foreign and domestic policies and specific issues of concern to the Soviet research and development community, provide new insights into the Soviet political process. But what is lacking is a concerted attempt to put these issues into the still broader context of the nature and evolution of Soviet political system. The closest that Parrott comes to this is in describing the general direction of the shifts which have occurred in Soviet official thinking over the past five decades, from a "traditionalist" line of thinking under Stalin to a more "non-traditionalist" orientation under Khrushchev and Brezhnev.

Given the fact that Parrott's analysis extends only up to 1975, some readers might ask whether Parrott's categorization of Kremlin debates still remains relevant today. The Soviets currently face a capitalism that is both dynamic and aggressive and an economic situation at home that deteriorated considerably in Brezhnev's last years. In his conclusion to the book, Parrott suggests that, rather than mitigate conflict over an issue which has been latent in Soviet communism for the past 50 years, these factors are likely to sharpen cleavages between "traditionalists" and "non-traditionalists."

MARK R. BEISSINGER

Harvard University

The Well-Ordered Police State: Social and Institutional Change through Law in the Germanies and Russia, 1600-1800. By Marc Raeff. (New Haven, Conn.: Yale University Press, 1983. Pp. ix + 284. $23.50.)

Nothing less than the making of the modern world is Marc Raeff's subject. His thesis is that the institutional and cultural foundations of the modern state resulted from the deliberate design of policymakers. The case is presented in three essays, a short one on the intellectual background to this transformation, a long one comprising about half of the book on the implementation of these new ideas in Germany, and a brief account of Russian attempts to copy the German examples. In this account the distinguishing feature of the *Polizeistaat* is that it is a positive government.

In the essay on the intellectual background Raeff emphasizes the influence of the Enlightenment faith in rationality as a means to progress. Specifically, he draws attention to the dawning realization that social and economic life was subject to scientific laws of some kind. In the interest of promoting sovereign power these scientific laws became the stimulus for the growth of government. Many writers, of course, have stressed the increase in regulation, centralization, and monopolization of governments of the period together with the influx of the bourgeoisie into administrative ranks. More important for Raeff's purposes is the changing outlook of those who governed.

This new outlook is detailed in his essay on the transformation of Germany. Based on a study of hundreds of *Landes-ordnungen* and *Polizeiordnungen*, this essay surveys religion, economy, manufacture, commerce, administration, society, and education. Positive government, Raeff contends, can be seen in the very way the law is presented. Preambles explaining and justifying each law were now affixed rather as Plato recommended in *The Laws*. One very frequently given justification, Raeff finds, is the need for *Aufhebung*, which he translates as lifting and transcending. In the aftermath of the Thirty Years War a premium was naturally placed on social stability, especially in the preservation of religious orthodoxy, but at the same time there was a great desire expressed in the *ordnungen* for economic and political change. In the course of his interpretation of the laws Raeff sets forth the implicit political theory of the dominant doctrine of mercantile cameralism. It consisted of two main precepts. The first was the feudal belief that the body politic is an organism. The other is the modern belief that society is a division of labor accompanied by a speciality of function. Raeff does not pause to assess the inconsistency of these principles. But it should be noted that the combination of organic social stability with the division of labor of modern political economy was the *Aufhebung* of Georg Hegel's political theory fully a generation after the end of the period of Raeff's study.

The book closes with a brief account of the failure of Russian attempts to import modernity by applying the German model. In the course of this chronicle Raeff opines that the ideas of cameralism had been largely implemented in the West. If that is so, it is not proven in this book. Raeff's interpretation of German society is based on the intentions expressed in the laws and not on what happened. Between the word and the deed falls the shadow. Reading the *ordnungen* and Raeff's exposition of them, a Martian historian would think that Germany consisted of dynamic, progressive, enterprising communities, each bravely exploring paths to the social, economic, and political future. Nothing could be further from the truth. At the time Germany was composed of stagnating feudal remnants in which politics was mainly confined to court intrigues over sexual liaisons, new wings to the palace, and the hire of mercenaries. Nor would our Martian colleague have any way of knowing from this account that cameralism and the *Polizeistaat* were strongest in the least progressive parts of Europe and weakest in the most progressive realms of Holland, France, and England. Still less would the Martian know that great thinkers like the Scots' Adam Smith and James Steuart, together with even greater German thinkers like Hegel and Karl Marx, dismissed cameralism and its ilk as the dead and of the past, as did the great Prussian reformers like von Stein.

M.W. JACKSON

The University of Sydney

The Women's Rights Movement in Iran: Mutiny, Appeasement, and Repression from 1900 to Khomeini. By Eliz Sanasarian. (New York: Praeger Publishers, 1982. Pp. xiii + 173. $24.95.)

In the past two decades feminists have criticized the social sciences for ignoring women and used that criticism as the basis for a new scholarship. The new scholarship focuses on the role of women in families, occupations, and local communities where the dispute is over the significance rather than the facts of their participation. But areas of social life abound where the mere activities of women cannot be described without evoking controversy. It is hard to imagine a more complex area than that of women and politics, and difficult to imagine a more challenging context in which to examine the relationship than in the Muslim world. For when we conceptualize political life, we tend to do so under the rubrics of power, authority, and public activity. What these categories signify seems totally inconsistent with our conventional notions of the ways in which Muslim women conduct their lives.

Eliz Sanasarian acknowledges the complexities of the relationship of Muslim women to Muslim polities in her book, *The Women's Rights Movement in Iran*. The book surveys the efforts of small groups of Iranian women to secure what they perceived as their rights to an education, employment, and political liberties. Ultimately, the book is unsuccessful in offering a deeply penetrating explanation for the development and impact of this century-old women's social movement. But the book's very failures are instructive. First, Sanasarian speaks of the limited amount of source material and the need to draw inferences of a dramatic order when studying the political activities of Iranian women. Second, Sanasarian attempts to find in the theoretical literature on political and social movements hypotheses and categories that might be usefully applied to the experiences of Iranian women. The problem of evidence is only compounded by a social scientific language deaf to the nuances of Muslim women's lives. Third, the multiple sociopolitical contexts in which men and women live at any one moment put any social scientific explanation to a very severe task. Is the description ever fully commensurate with the realities? New scholarship—especially that which attempts to break new ground and to explore the unfamiliar—seems often out of harmony with established standards. But we need to press on with these efforts to enlarge our sense of human experiences—of men and women—recognizing that progress is not inevitable and moreover, that failure is necessarily part of the learning process.

DONNA ROBINSON DIVINE

Smith College

Toward a Human Rights Framework. Edited by Peter Schwab and Adamantia Pollis. (New York: Praeger Publishers, 1982. Pp. x + 258. $26.95.)

In this book the editors present a number of case studies that elaborate the thesis, presented in an earlier book, that conceptions of human rights vary widely among nation-states because of differing socioeconomic, cultural, and ideological contexts. They consider the emphasis on universal human rights that is embodied in the Universal Declaration of Human Rights to be "grossly political," although they believe that insuring minimum life needs might provide the core of a future universal consensus on human rights. Even if rights are defined differently for liberal, socialist, and third-world states, or yet more specifically are defined differently for each state, one of the major conclusions which the editors draw from the case studies is that all states violate the rights of their citizens.

Not surprisingly, given the editors' commitment to a high degree of relativism in the definition of human rights and the absence of any more elaborate framework in the book, the case studies are an extremely mixed bag. Given that most of the authors writing on socialist and third-world states are from liberal-democratic capitalist countries and were allowed by the editors' relativism to express their own values, many of them make judgments that many other observers, probably including the editors in some cases, would call "grossly political." I would argue that all conceptions of human rights are inherently (although not "grossly") political, and that relativism justifies rather than eliminates this situation.

The high points of the book are John S. Gitlitz's chapter on Nicaragua and Om P. Gautam's chapter on India. Although somewhat dated because it covers events only through early 1982, Gitlitz's treatment of the difficulties encountered in maintaining human rights in a situation of revolutionary-counterrevolutionary confrontation is balanced, realistic, and theoretically sophisticated. It would make excellent reading for anyone who wants to get behind the Reagan administration's rhetoric on Nicaragua and understand the true complexity of the situation there. The outcomes of the conflicts over the definition of political pluralism, mixed economy, and human rights in Nicaragua are in doubt according to Gitlitz, and I believe that these outcomes will be important influences on the definition of human rights in other socialist, third-world countries. Gautam discusses the influence of both liberal and socialist conceptions of human rights in India but demonstates that political instability and economic underdevelopment place strong constraints on the effective implementation of either political or economic rights.

The low point of the book, by a substantial margin, is Peter Schwab's chapter on Ethiopia. Schwab's commitment to meeting minimum life needs has already been discussed, and he dedicates his share of the book to friends who "believe that life should be kind and gentle to others." Strangely, then, his chapter is an apologia for the extreme violence exercised by the Ethiopian military regime against its opponents of both the Right and the Left and an attack on leftist critics of this violence. Claiming to base his argument on the ideas of Frantz Fanon and Albert Memmi, Schwab states that because violence is virtually necessary in a genuine social revolution, leftist observers of Ethiopia should celebrate rather than condemn the occurrence of extensive violence in that country because it was a part of making or defending a genuine revolution. He fails to point out that Fanon and Memmi were discussing the colonial situation specifically, and that Fanon advocated violence by the oppressed against the colonizers in the process of making a revolution rather than regime violence against large segments of the populace in the postrevolutionary period. Schwab dismisses Eritrean and Somali leftists as counterrevolutionary by virtue of being ethnic secessionists, and the Ethiopian People's Revolutionary Party (EPRP) as a party composed of "foreign educated Leftists of the middle class" which "misjudged the moment, and in its own peculiar way placed itself in a counterrevolutionary role" (p. 192). Thus, he continues, in order to acquire human rights for the oppressed masses and "develop social democracy throughout Ethiopia," the regime had to exterminate these groups through violence and should be congratulated by all true leftists for doing so.

What Schwab defends is not a genuine state-specific Ethiopian conception of human rights, but a conception held by a highly authoritarian military regime and opposed by many Ethiopians, including those on the Left. In my opinion, Schwab's defense is both empirically absurd and morally indefensible in terms of his own values. He ignores the valid generalization that economic and political rights must to some degree develop together.

James R. Scarritt

University of Colorado, Boulder

A Woman's Issue: The Politics of Family Law Reform in England. By Dorothy M. Stetson. (Westport, Conn.: Greenwood Press, 1982. Pp. xiii + 278. $29.95.)

Dorothy Stetson presents a legislative history of reform of the English doctrine of coverture. Coverture provided that a married man and woman became as one, the man. The married woman's legal rights were almost obliterated. *A Woman's Issue* is an interesting, useful book for the fields of public policy, legislative process, constitutional law, British politics, and women's studies. Stetson presents evidence well and writes competently. Readers' guides such as a table of abbreviations are helpful. An enormous amount of work is evident, and *A Woman's Issue* has much to recommend it.

Stetson's dependent variables include 10 policies (seven statutes) in the area of divorce and property reform from 1857 to 1970. Her independent variables are dichotomized: feminists or modernization? She analyzes their role in resolving conflict over values, drawing on the analytic paradigm of E.E. Schattschneider among others.

From the analysis emerges a complex arena described as administrative, legislative, and administrative/legislative. Stetson demonstrates increasing specialization and regularization of the decision-making process and its relation to action on reform. Feminist individuals, organizations, and organizational representatives influenced egalitarian decisions by moving values conflict into the legislature.

Here the problems begin, suggesting a need for more diachronic analysis. Nowhere is feminism defined. Alison Jagger contends that early twentieth-century feminism in the United States meant "the uniqueness of women, the mystical experience of motherhood and women's special purity" (*Feminist Politics and Human Nature*, Rowman & Allanheld, 1983, p. 5). Stetson seems to mean otherwise, but never says what. Further, she may often not be observing feminists, as women's rights advocates are not necessarily feminists. Similar problems appear in other recent studies of this genre. To avoid studies purporting to measure feminism but not doing so, we need to address definitions and validity.

Modernization is not mentioned again after page 3. Implicit operational definitions of the concept are nonetheless evident and include: the role of the state, class struggle, military and economic crisis, structural changes such as suffrage expansion, the legal profession, and social forces leading to gender role adjustment. Also evident is the part played by the Anglican church hierarchy and women's organization, especially interesting are the governmental roles of some church officials and declining public religious identification. Partisan context was demonstrably influential, despite Stetson's claim that it was not (pp. 223-224). An issue does not have to become one party's bailiwick to be affected by partisan context. Although Stetson states that the formal legislative process is not of interest in policy enactment (p. 13), her findings suggest otherwise (see pp. 72-73, 83, 89, 101, 111, 117-118, 123, and 124). All these variables are salient, although not consistently favorable to reform.

From 1851 to 1935, the incremental "redistribution" of values often favored men and probably, in the short run, conferred more burdens than benefits on women. Over the long run, four generations of women, redistribution became more worthwhile. The achievement was role equity, not role change. Later, the Matrimonial Proceedings and Property Act of 1970 authorized "the court to protect the income of middle-aged dependent wives . . . divorced against their will, . . . unlikely to remarry, and [with] little opportunity to be employed" (p. 153). Stetson comments that "such protections, rather than guaranteeing economic rights, reinforce the dependency of married women" (p. 153). Today, opposing protection for equality is almost *de rigour* (see Judith Baer's *Equality under the Constitution*, Cornell University Press, 1983). The hard line does not erase decramental problems with incrementalism.

Stetson illuminates these problems, but not always directly: Delayed gratification is not delay over 120 years or over an individual's life span. The National Organization for Women's refusal in the United States to support compulsory selective service registration for women, without equal rights, is analogous and instructive, as is the National Consumers' League's support for protective labor legislation for women, without equal rights. Neither approach confronts underlying, intact gender role differences.

Neither, apparently, did the British activists; missing is the detailed analysis of activists found in Jo Freeman's *Politics of Women's Liberation* (McKay, 1976). I was never certain about the dimensions of influence attributed to British activists, and linkage between pressure and output was subsumed in the overall description.

Stetson assumes too much. Terms such as "backbencher" and "place in private member ballot" are not defined. I found shortsighted the tacit acceptance that poverty and lack of (formal) education among working-class people were offensive (see pp. 40, 57). I felt similarly about the tacit acceptance of the offense of legally classifying women with "children and idiots" and references to women's "handicaps" (pp. xi, 3, 5, 9). As researchers we learn from our mistakes and grow through our accomplishments. *A Woman's Issue* demonstrates and contributes to scholarly development; its errors may do the same.

SARAH SLAVIN
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The Arab Balance of Power. By Alan R. Taylor. (Syracuse, N.Y.: Syracuse University Press, 1982. Pp. xiv + 120. $22.00 cloth; $11.95 paper.)

Alan Taylor of the American University writes a highly readable and succinct account of contemporary Arab politics. *The Arab Balance of Power* should be welcomed by undergraduate and graduate students of the contemporary Middle East.

The book is divided into eight chapters in addition to 21 appendixes. Chapter 1 provides a brief yet very good overview of unity and diversity in the 22 Arab states of the Arab world. Chapter 2 traces the history and development of the present Arab system. Chapter 3 deals with the rivalries between the Hashimite and Saudi-Egyptian bloc, the revolutions in Syria, Egypt, and the formation of the United Arab Republic (UAR). The ebb and flow of the Pan Arab movement is lucidly analyzed, and the difficult task of deciphering the continuously shifting alliances and policies is well done. Chapter 4 deals with the breakdown of the Arab system—the collapse of the UAR and the Arab defeat in 1967.

The causes of cooperation and conflict are dealt with. Although Arabs want unity, every time a unification scheme is attempted it fails. Why? There, according to Taylor, lies the basic dilemma.

Chapter 5 deals with the years after the 1967 defeat until Sadat's visit to Jerusalem. In a section titled "The Search For Solidarity," Taylor focuses on the era following the October, 1973 War and dominated by Saudi Arabia, or *Al-Haqaba Al-Saudia*. This era is dominated by the theme of diplomatic maneuvering as opposed to open, armed conflict with Israel. "Money talks, and it must surely talk loud when there are billions of it," as the late Malcolm Kerr commented, "the Saudi money whispered." Through its contacts, helped along by its financial resources, Saudi Arabia tried to maintain its role as a stabilizing power and attempted to harmonize relations among different Arab states. This role was thrust upon the Saudis by circumstances; they neither wished the role, nor were they comfortable in it. Yet others have suggested that *Al-Haqaba Al-Saudia* was an era of deradicalization of Arab politics masterminded by rich Arab and marked by the drawing of the Arab world farther into the American sphere of influence; the encouragement of consumerism, as in the case of *Al-Infitah* in Egypt; the papering over of serious economic, social, and political problems; and the encouragement of *Al-Islam Al-Rasmi*, sometimes referred to as "Petro-Islam" or American Islam, at the expense of popular Islam.

Chapter 6 deals with the polarization of Arab politics after Sadat's visit to Jerusalem, whereas chapter 7 deals with regional disputes.

Perhaps the most interesting chapter is the concluding one, "The Arab Dilemma." To Taylor, this dilemma lies in the interplay of universalist and particularist forces. On the one hand, the Arabs have a common cultural heritage and a common historical experience and, on the other, a diversity of regional and geographic circumstances. The Arab state system is alive and well. Its continuation is in the interest of the people and the regimes of these states. People and governments pay lip service to Arab brotherhood and Arab unity as long as it's not practiced. This hypothesis may be easily tested. Ask any Egyptian trying to get a visa to Saudi Arabia or an Arab university professor looking for academic tenure in Kuwait.

Perhaps Arab politics are a long way down the road from *The Arab Balance of Power*. It is possible that secularists and Pan Arabists have already lost the day to the "kids in sneakers" who march to the beat of a different drum.

The book would have been greatly enriched if it had utilized some of the literature published in Arabic, such as the work on Arab unity by Saad Eddin Ibrahim, the Arab system by Ali Dessouki, Saudi Arabia's foreign policy by Chassan Salameh, and a number of attitudinal studies by Faisal Al-Salem, Ahmad Dhaker, and Tawfic Farah.

TAWFIC E. FARAH

Journal of Arab Affairs

China Turned Rightside Up: Revolutionary Legitimacy in the Peasant World. By Ralph Thaxton. (New Haven, Conn.: Yale University Press, 1983. Pp. xxi + 286. $27.50.)

Scholars have devoted consideration energy to attempting to understand the Chinese Communist party's success in establishing the People's Republic of China. Most agree the CCP based its revolution on the rural peasantry, but debates continue over whether the CCP obtained peasant support through social and economic appeals or through the mobilization of a new nationalism in the wake of the Japanese invasion of China.

Thaxton enters this debate and attempts to turn the discussion "rightside up" by deemphasizing the role of the CCP and stressing the revolutionary proclivities of the peasants themselves. Rejecting the concept of a party-led peasantry, Thaxton argues the "Chinese Communists had to heed a set of social concerns posed by the poorest peasants if they were to be accepted" (p. 98). He begins with a description of the North China border region filled with a self-reliant peasantry ready to rebel when the state threatened survival. After warlords replaced Imperial government, their depredations and surtaxes combined with the unilateral landlord abrogation of customary tenant rights led to horrendous poverty and famine in North China. Through self-help, remedialist protest, and opposition, peasants attempted to solve their problems well before the Japanese invasion. In the fourth chapter, Thaxton argues that "the peasants who asserted themselves in the border region were the comparatively insecure landless tenants and hired hands," not "the secure landowning 'middle peasants'" (pp. 94-95). After a case study of a village, Thaxton turns to analyses of the peasants and the CCP during the wars against the Japanese and the Guomindang, or Nationalist party, arguing the CCP had its greatest successes in outlying villages least affected by the Japanese or Guomindang.

In researching this book, Thaxton has delved deeply and widely using numerous Chinese Communist documents, short-term field research in China, and extensive reading in both China studies and the social sciences. Yet, I found the book both disappointing and unconvincing. First, Thaxton does not marshal evidence satisfactorily. Repeatedly, he simply asserts controversial points or backs them with a footnote to a Chinese historical journal published between 1974 and 1976, when most Chinese "history" used historical settings to comment on contemporary politics. Attempting to show the "bitter class feelings of poor folk," Thaxton quotes at length a song (pp. 132-134) which says the landlord survives "With the help of the Japanese forces," thus actually strengthening the peasant nationalism argument that he attempts to debunk. In the case study, the peasants do not rebel successfully until the CCP becomes involved (p. 146) and again only CCP intervention saves the peasants (pp. 154-155).

Stereotypes, too, fill this book. When Thaxton refers to "peasants," the reader often cannot decipher whether Thaxton refers to individuals, families, or villagers, and too frequently Thaxton raises the image of the "noble peasant" (my term), a concept just as useless and dated as the "noble savage" of yesteryear. He uses the Guomindang as a wicked stereotype and fails to draw distinctions between the central party and government apparatuses in South China, the relatively autonomous northern warlords affiliated with the Guomindang and local party members with varying connections to the central party apparatus. Repeatedly, he asserts the Guomindang could have solved the peasant problem, a dubious proposition which inflates the Guomindang's power and capabilities. "Western scholars" as strawmen pepper the book; "Western views" obtain 28 citations in the Index. Because one former Catholic missionary from North China used a conspiracy theory to explain CCP success (p. 152), the Catholic Church receives an undocumented, emotional lambasting (p. 159). Thaxton also draws a very sharp dichotomy between the evil Confucian Great Tradition of officials and landlords and a moral Buddhist Little Tradition of peasants. In China, a continuum rather than a dichotomy describes the cultural differences between rich and poor. If the peasants were anti-Confucian, how would Confucian argumentation, which Thaxton documents the landlords using, have appealed to them? In fact, Chinese folk religion is much more syncretic than Thaxton allows, combining Confucian, Buddhist, Daoist, and other elements.

Finally, the book presents many facts and analyses without making clear how they relate to the book's arguments. Perhaps this disappointing book might have made a useful contribution to an important, exciting topic had the author written with more subtlety and consistency.

J. Bruce Jacobs

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The Japanese Diet and the U.S. Congress. Edited by Francis R. Valeo and Charles E. Morrison. (Boulder, Colo.: Westview Press, 1983. Pp. xii + 212. $12.00.)

This volume, a product of a conference organized by the U.S. Association of Former Members of Congress and the Japan Center for International Exchange, is designed to increase the understanding that Americans have of the Japanese Diet and that the Japanese have of the American Congress. To this end, six leading American students of the Congress and six Japanese scholars and legislative staff members were commissioned to write brief papers addressing six themes common to both legislatures: an historical perspective, the role of political parties, the structure of the legislative process, the activities and backgrounds of individual members, the legislative role in budgetary policies, and the legislative role in foreign policy.

The first thing that needs to be said about the 12 articles produced by this plan is that they are not written for a scholarly audience. Each article is heavy on description, many are concerned almost entirely with rules or with constitutional and statutory provisions, none present any new empirical data, and, although most are clearly informed by the scholarly literature on the two legislatures, they allude to it only in passing while ignoring entirely the existing body of work that has looked at legislatures comparatively.

Students, or even informed observers, of the American Congress will find nothing new or particularly insightful in the six articles on the Congress, and those with a similar knowledge of the Diet will have the same reaction to the six articles on that legislature. What we have here then is really two books in one with each part written for a different audience. The essays on the American Congress are for a general audience that knows little about that institution, whereas the six essays on Japan are for a general audience similarly uninformed about the Diet. In this limited context, the book proves to have some utility.

I found the chapters on the Diet quite informative. The dominant role of party and bureaucracy in the functioning of that institution is well-stated, and the similarities with several European parliaments are quite marked. The similarities between the daily experiences of Diet members and those of their American counterparts are also striking and fit well with data from a variety of legislatures. This suggests the manner in which the book will be read and used by political scientists: for pieces of information that will confirm or question tentative generalizations derived from one's knowledge of other legislatures about how legislative institutions in general operate.

The book's main fault is not of the authors' making—they have by and large done a good job following a mandate that must have been to assume no previous knowledge and to avoid theory and data—but of their editors, who miss the opportunity to make the volume a more useful contribution to comparative legislative research.

Thus, while they went to the trouble of commissioning articles dealing with parallel themes, they organize the book not by those themes but by country, so that the six articles on Japan come first and the six on the Congress afterwards. Rather than juxtaposing the chapter on, for example, the role of the Diet in making Japanese foreign policy with that on the Congress' foreign policy role so that the comparative lessons can be more readily drawn, these comparable chapters are separated by almost 100 pages. Also, the book is desperately in need of a summary chapter that would make explicit the comparisons embedded in the various chapters. Something as simple as an inventory of differences and similarities would have provided a useful guide to further research and inquiry and thus made the volume as valuable to political scientists as it is to parliamentarians and journalists.

MICHAEL L. MEZEY

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The Political System of the Federal Republic of Germany. By Klaus von Beyme. (New York: St. Martin's Press, 1983. Pp. xiii + 209. $25.00.)

"There are few countries in Europe," writes Klaus von Beyme in the Preface to this book, "where the cleavages between metatheoretical schools involving comprehensive policy-outlooks are so deeply entrenched as in Germany since the late 1960s." Von Beyme, one of the Federal Republic's best known and most prolific political scientists, presents us with an analysis of the West German political system that seeks to take account of the perspectives and critiques of both major schools—systems theory and neo-Marxist—while presenting his own reinterpretation of the workings of the system's institutions. He supports his argument with numerous comparisons to other liberal democracies and includes brisk discussions of many proposed reforms. All this could be an exciting undertaking, but it is entirely too much to attempt in a slender 200-page volume, and the reader is repeatedly left dissatisfied when tantalizing themes are briefly touched upon and then quickly abandoned so that the author may move on to the next subject.

The present work appears to be largely a translation of von Beyme's 1981 German book of the same title, although this fact (and the name of the translator) is not made explicit. Thus, in spite of its 1983 copyright date, the book is essentially a study of the West Germany policy as of the late 1970s. Readers seeking an extensive discussion of the fall of the Schmidt government, the return to power of the CDU/CSU, or the continuing advance of the Greens will be disappointed. Although the words are English, the syntax and idiom of the translation are often German, and the combination of the two produces sometimes amusing and occasionally incomprehensible results. The book is also studded with typographical errors.

These flaws are particularly unfortunate because von Beyme deals with a number of important topics that are neglected in most American textbooks on West German politics and introduces the reader to criticisms and reform proposals from the West German literature that have received scant attention here. The discussion of party finance, the charges from left and right that the Federal Republic has become an *Unternehmerstaat* or a *Gewerkschaftstaat*, the debate surrounding "Concerted Action," and von Beyme's charge (not, to my mind, sufficiently documented) that the Federal Constitutional Court has "deeply interfered with the prerogatives of other constitutional powers" (p. 186) are examples of important themes in the book that deserve still more development than they receive.

The bibliographies at the end of each chapter, which have been extended to 1982, provide the scholar with a valuable guide to the German-language literature on West German politics. Apart from these, and whatever stimulation that might lead to further exploration of some problems, the serious researcher will find the treatment of most subjects to be too fleeting to be of much use. On the other hand, I fear that the book assumes too much previous knowledge to be suitable for use as an undergraduate text. Von Beyme attempts too much in too narrow a space and gives us a "reinterpretation" of West German politics that, in spite of its provocative assertions and numerous insights, lacks focus and, too often, adequate elaboration.

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The Egypt of Nasser and Sadat: The Political Economy of Two Regimes. By John Waterbury. Princeton, N.J.: Princeton University Press, 1983. Pp. xxiv + 477. $45.00, cloth; $12.50, paper.)

One of the strengths of this thorough study of post-1952 Egypt—and one that makes it important for students of the Third World in general—is the extent to which it is permeated by theoretical questions. Much of Waterbury's analysis is in the context of the *dependencia* approach, which, however, he applies quite critically. Some of his conclusions—for example, that "neglect by the core" is what is conducive to "pauperization" (p. 22)—fly in the face of the usual *dependencia* analysis. The last section, which deals specifically with relations with the outside world, argues that dependency can be imposed by socialist as well as by capitalist powers and that, instead of having wealth drained off to the "core," Egypt has been able to use its geostrategic position "to extract tribute" (p. 405). Yet some passages carry the ring of more orthodox dependency analysis—like Waterbury's characterization of Sadat as the champion of the "compradors" and of Mubarak (whose regime gets minimal treatment in a study that was essentially completed before his predecessor's death) as possibly favoring the "national bourgeoisie" (p. 175).

The longest section of the book concentrates on the emergence of "state capitalism" under Nasser and of Sadat's shift to the "open door" policy, encouraging the private sector and foreign investment. It is emphasized that the roots of Egypt's currently growing dependency go back to the Nasser period—to an "inclusivist" and "populist" program aimed "at redistribution of wealth that, ultimately, derailed the process of accumulation" (p. 10). However, Waterbury concludes that what growth occurred under Sadat (accompanied by growing inequity) was not the result of the open door but of the fortuitous upsurge of invisible earnings, like remittances, that enabled Sadat to benefit from some of Nasser's policies, which thus "were never given their day in court" (p. 204).

A major portion of the volume focuses on class structure. Waterbury concludes that in Egypt the state is largely autonomous of class interests, with the "traditional path to economic status lying through access to political power" (p. 232). He sees the emergence under Nasser of a managerial "state bourgeoisie" whose corrupt ties to the private sector developed on a new scale under Sadat.

Waterbury makes an important contribution to the debate about the role of the rural bourgeoisie, those whose small holdings survived land reform and to whom some scholars trace the origin of the post-1952 political elite. Although he agrees that these *kulaks* have thrived, he believes that the elite is of much more heterogeneous background and has not in fact been responsive to their interests.

One relatively short section focuses on the evolution of the political system, to which Waterbury applies the corporatist model. A picture emerges of Nasser as being ahead of most of his associates in wanting a real mobilization party—and "want[ing] to reach out to new, underprivileged constituencies to sustain the socialist transformation" (p. 332)—but too overwhelmed by immediate concerns and limited by "centers of power" to push in that direction for long. The picture of Sadat is of one who temporarily introduced a limited element of political liberalism but ultimately retreated in the face of threats to his position to the kind of repression he had rejected.

This book will inevitably provoke much debate from proponents of both dependency and conventional or modernization paradigms. But it is undeniably one of the most important works on contemporary Egypt (a few inconsequential factual mistakes notwithstanding). Waterbury combines a truly impressive amount of original research, heavily involving Arabic sources, with knowledge of an extensive body of theoretical literature.

GLENN E. PERRY

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New Military Politics in Latin America. Edited by Robert Wesson. (New York: Praeger Publishers, 1982. Pp. x + 230. $26.95.)

In spite of the title of this book, the "new" military politics in Latin America are not new, dating as far back as 1964 in Brazil, so that the apparent deep, permanent involvement of the military in politics has been the object of careful, sustained analysis. Nonetheless, some new contributions are made in the generally high-quality essays of this edited volume. Eight essays on the involvement of the military in the politics of seven South American states (Argentina, Brazil, Chile, Colombia, Peru, Uruguay, and Venezuela) constitute the most up-to-date survey of the subject. Two introductory and two concluding essays of a general nature complete the volume, although the division of labor between these four essays is often not clear and appears to reflect the exigency of accommodating diverse interests of conference participants in preparing these proceedings.

The four general essays also relate awkwardly to the eight case studies. Whereas the four general essays usually integrate examples from the Caribbean/Central America as well as South America, the case studies focus exclusively on South America. Because variations from, and exceptions to, the new military politics in the subregions of Latin America are not explained systematically, a firm basis is not provided for assessing the future of the phenomenon. In fact, there is considerable disagreement among the various authors over whether the new military politics are being transcended, or whether this simply is being expressed in new guises. Although democratization in Argentina postdates the book, *abertura* in

Brazil does not, yet the reader learns more of the dynamics of the Brazilian political opening than of its prospects. There are likewise only tentative suggestions about why Colombia and Venezuela have been spared the active military involvement characterizing their neighbors and uncertainty regarding whether this state of affairs will continue.

Because the case studies are uniformly rich and sophisticated, these shortcomings may reflect the intractability of the subject matter more than any inadequacy of the authors. In particular, the complexity and idiosyncracies of each case study make it extremely difficult to generalize about both the past and emerging characteristics of the new militarism. Generalizations made in the four opening and concluding essays, while usually insightful, do not seem integrally linked to, or to grow out of, the case studies.

Accordingly, an important contribution of the book may be to advance a dialogue between the general and the specific in assessing the new militarism, even though a viable synthesis remains elusive. Because democratization appears to be proceeding at an uneven pace along with militarization, which is complicated further by subregional and national peculiarities, continuing reassessment of the new militarism is advisable.

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Governments, Markets, and Growth: Financial Systems and the Politics of Industrial Change. By John Zysman. (Ithaca, N.Y.: Cornell University Press, 1983. Pp. 358. $24.50.)

Zysman's well-written comparative study of industrial policy in five Western countries establishes the important role that financial institutions play in shaping the state's response to industrial change. Indeed, his "central proposition is that in advanced countries the structure of the domestic financial systems helped shape the politics of industrial change in the postwar years" (p. 54; see also pp. 16, 285, 286, 298, 300; for different or extended formulations, pp. 80, 81, 274).

The framework for the analysis postulates three scenarios: (1) capital market-based financial systems and company-led adjustment strategies; (2) credit-based financial systems with government-administered terms and state-led adjustment processes; (3) credit-based financial systems with dominant financial institutions and negotiated adjustment processes (p. 18, chap. 6). Financial systems in the United States fit the first scenario, those in France and Japan fit the second, and those in Germany the third; Britain's policy remains ambiguous.

The economic rationale for state industrial policy is stated clearly. "Neither the competitive position of firms in crucial sectors nor the comparative advantages of the advanced countries will be fixed. Both can be created by the strategies of firms and by government policies" (p. 36). Government has a role in industrial policy because industry changes cause displacement costs that are in the social interest to minimize, but firms often externalize these costs, perhaps less to workers than to cities, counties, homeowners, and other owners of the infrastructure that is threatened with obsolescence by abrupt industrial change.

Zysman's analytical framework suffers from assuming a basic dichotomy between the market on the one hand and the polity on the other. This assumption disregards the state as an entrepreneur. However, the public-enterprise sector of France, Britain, and Germany is considerable, and even the United States has again recently proven with Conrail that America can be successful at private enterprise. Because public enterprise is often used as an instrument of industrial policy, it cannot be ignored in a study on industrial policy, and assertions to the contrary (pp. 71, 76, 79, 86) miss the point.

The state is not only an entrepreneur and producer of goods and services, but also the single largest producer of institutions. In this respect, the different states vary considerably, and that variance is not explained by the market-state dichotomy. Although the countries with a tradition of codification (notably France and Germany) produce a steady flow of newly adapted and malleable institutions (p. 165), the common-law countries, Britain and the United States, face serious difficulties that leave them without the very instruments that would allow for industrial policy (pp. 210, 221, 230, 307).

In its capacity as producer—either for the market or for the political environment in which institutions are placed—the state can be an allocator or a distributor of resources. Which role will predominate depends on the incentives faced by those who make state political decisions. The account shows clearly that politicians in both the United States and Britain are allowed to squander the tax base (p. 279). But in Japan and France as well as in Germany, the impression is different. In *The Rise and Decline of Nations* (Yale University Press, 1982), Mancur Olson accounted in part for the institutional difference by linking the duration of democratic traditions with economic growth. Democracies constrain the time horizon of their politicians, which is why investments in the tax base are small. In France, however, an elite bureaucracy remains removed from democratic control. Democracies can neutralize interest-group pressure by opening their markets or entering into customs unions. It was this policy of joining the Common Market that France impressed upon an unwilling commerce and industry (pp. 134-138).

The different history of ideas should matter, too. The British Ricardian tradition focused on exchange, while on the Continent, mercantilism—and even more so, cameralism—as well as economists such as Friedrich List emphasized industrial development.

Zysman's account of the financial systems of France, Britain, Japan, Germany, and the United States is the stale result of ploughing through almost exclusively English-language secondary literature. Because the book is not supposed to be a reference work on financial systems, but rather a collection of case studies highlighting a common theme, the various misrepresentations (pp. 143, 146, 173, 207, 255, 258, 261, 291, 292, 302) that will invariably occur when such a method is used should not unduly distract us from the main theme: the importance of financial institutions for the state's response to industrial challenges. The reader who labors through these accounts will perhaps ask what makes these institutions work. Why are they allowed to operate in this manner and not in another, perhaps more efficient, way? Why, on the one hand, is the British cabinet minister so little interested in monitoring the nationalized industries (p. 202)? Why does he leave the task to a bureaucracy ill-suited for the purpose? Why, on the other hand, is the French bureaucratic elite interested in modernization? And why are they allowed to obscure the costs of their decisions (pp. 135, 166)? Institutions are not a given, they change as individuals shape them in pursuing their ends. These ends may not be obvious: The challenge is to uncover them in order to present a well-rounded theory of the institutions analyzed. *Governments, Markets, and Growth* is a welcome beginning.

Zysman is careful to avoid exaggerated claims. The problem here is not with the case-study approach, although its usefulness is restricted due to incomplete conceptualization. Rather, we are unable to assess the impact of, for example, French interventionism because we do not have a control case to study the absence of intervention in a similarly constituted country. The success or failure of French industrial policy cannot be measured long as we lack an estimate of the opportunity costs of these policies.

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International Relations

Crisis Bargaining and the Arms Race: A Theoretical Model. By Pierre Allan. (Cambridge, Mass.: Ballinger Publishing, 1983. Pp. xxii + 155. $22.50.)

This book promises much, a grand structural, mathematical integration of the evolution of conflict among states including an explanation of the intensity and timing of these conflicts. The theory constructed in the major portion of the book is tested in later chapters against U.S.-Soviet post-World War II relations.

Allan's theory of an evolving cyclic conflict among states consists of six sub-models as follows: (1) A measure of time (τ) or of the pace of events wherein the duration of, and interval between, historical events (x,y) depends on the number and intensity at which they occur as well as on chronological time (t) (Allan calls this "diplomatic time" as distinguished from proper or chronological time); (2) a causal (deterministic) relation between the conflict behavior (x,y) manifest by states, the diplomatic climate (DC), and balance of power (BP) in which they function; (3) a causal relation between diplomatic climate (DC) perceived by a state and the cumulative history of conflict behavior (x,y); (4) a causal relation between the balance of power (BP) among states, the balance of their resolve (R), and the power capabilities (PC) of various states; (5) a causal relation between the balance of resolve (R) and the history of conflict behavior (x,y, past x, past y); and (6) finally, a military arms race model in which the power capability of a state depends on its own GNP, the diplomatic climate, and other states' power capabilities.

If these structural relations appear complicated, they are. There are 9 equations in 12 variables. All variables are endogenous except for chronological time, t, and the GNP trends of competing/cooperating states. In principle, the entire time-trajectory of international conflict is derivable from GNP trends. Allan's model has no recourse to a nation's objectives, its utility, or preferences. Conflict is not the result of a maximizing or objective-seeking process. Instead, the course of conflict is "mechanically" determined as, for example, in Richardsonian models of arms races. Thus, Allan's model lacks a crucial component to the volitional process it aspires to represent, namely some goal-seeking, satisficing, or maximizing behavior by states. At the same time, some components proposed by Allan appear superfluous and nonobservable. For example, the distinction between ordinary time and diplomatic time is unmeasurable, as is the "balance of resolve." These shortcomings are not fatal to Allan's theory, however, because the unobservable causal relationships are "folded into" relationships that are measurable, and his full-blown model is clearly provocative for deriving inferences from alternative assumptions about individual states behavior.

To extract specific predictions from his system, Allan is obliged to assume specific functional forms for the causal relations postulated. For example, to indicate how a state's power capability, PC_i, depends on the power capability of other states PC_j, Allan assumes a negative exponential relationship:

$$PC_i \doteq e^{a_i - C_i \mid PC_j};\ a_i, C_i = \text{parameters}$$

The analytic properties of the international system such as its stability and dynamics will depend crucially on the assumed functional form. More specifically, the claim that conflict may evolve in cycles depends critically on such specific conjectural assumptions as to the quadratic nature of equilibrium conditions for individual states.

Thus, Allan's conjecture that the international system may exhibit multiple equilibria and regions of instability is a great distance from any positive explanation of the time profile of conflict. The empirical effects toward the end of the book do little to confirm his conjecture. But the equation systems developed in the theoretical sections of the book do lend themselves to further analytic development and possibly simulation. For the specialist with an interest in exploring possible very long-run implications of myopic individualistic decisions by states in a system, Allan's models will prove useful.

MARTIN MCGUIRE

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Israel, the Middle East, and U.S. Interests. Edited by Harry S. Allen and Ivan Volgyes. (New York: Praeger Publishers, 1983. Pp. vii + 167. $27.95.)

The Persion Gulf and American Policy. By Emile A. Nakhleh. (New York: Praeger Publishers, 1982. Pp. xvi + 151. $22.95.)

Few regions of the world claim as much attention from American policymakers as the Middle East, and two new books attempt to put American Mideast policy in perspective. Emile Nakhleh's book focuses on the Persian Gulf, and he begins with the assumption that the Gulf's problems should be viewed through the perceptions of Gulf residents. Too often, he argues, Gulf issues are viewed primarily in East-West terms. Although those concerns are not unimportant, Gulf issues have their own dynamics best understood by the people who live there. Consequently, Nakhleh bases much of his research on interviews with political leaders, elites, intellectuals, and others in the Gulf region who often go unnamed. These and other documentary sources explore the research questions, What is U.S. Gulf policy? What are its successes and failures? and, What are the real components of regional security in the Gulf?

Nakhleh says that American policy has been to protect both American access to Gulf oil and the Gulf from Soviet expansion. After 1973, these goals emphasized closer U.S.-Gulf military and economic ties, regaining petrodollars, and obtaining bases for the Rapid Deployment Force. Nakhleh concludes that although Gulf regimes are sympathetic to these American policy goals, the American failure to obtain Israeli concessions on the Palestinian homeland issue precludes closer U.S.-Gulf ties. Regarding regional security, Gulf leaders share the American fear of Soviet expansion and are somewhat interested in an American military guarantee. Again, the Palestinian problem would have to be solved for Gulf leaders to cooperate with the United States free from fears of Arab criticism. However, real stability cannot come to the Gulf until the area's autocratic, familial regimes open themselves to some meaningful form of public participation in governance. Until then, the regimes will be vulnerable to both internal and external threats.

Although it attempts to explore American interests more broadly in the Mideast, *Israel, the Middle East, and U.S. Interests* by Allen and Volgyes contains a diverse set of essays. The first suggests the general pro-Israeli and pro-American tone that a number of the chapters share. In it, Todd Endelman denies the contention that both Jews and Arabs have equal claims to the land of Palestine. By citing the centrality of the land to

Jews on religious, cultural, and secular bases, Endelman says that the Jewish claim is the stronger of the two. Also suggesting the pro-American tone, chapters by Haim Shaked and James Leonard applaud the parties involved in the Camp David Accords and urge a more active American role in encouraging similar negotiations.

A number of chapters focus on the military security issues of the region. Alvin Rubinstein and Vernon Aspaturian agree future Soviet policy in the area will follow a policy of realpolitik rather than any grand design or ideological crusade. Such a policy will constitute a danger to both the region and to American interests. Trond Gilberg offers a different approach by examining the activities of Eastern European actors in the region. He notes the Ceausescu regime in Romania has been able to play the role of an "honest broker" In both chapters 7 and 8, "The Correlation of compromise. Two essays go right to the heart of the American security concern. Amos Perlmutter endorses the presence of American military bases in the area to discourage revolutionary pressures and to give the United States the ability to intervene quickly to protect vital interests. Michael Handel points out that Israel would be a great location for such bases, and the United States could learn much from the Israelis in the military and intelligence fields.

Four essays go beyond the pro-American and pro-Israeli stance embodied in the chapters mentioned previously. Abdul Said and Alain Sportiche bemoan the distorted imagery and perception that mark Arab and Israeli views of the other. Nikke Keddie goes further by saying American policymakers ignore the realities of the area and produce unsuccessful, even counterproductive, policies. The other two essays offer a functional approach to Mideast peace by suggesting agricultural means to bridge the gap between the peoples of the area. Cyrus McKell discusses Israeli rangeland policies that could benefit all the region's states, whereas Don Gustafson makes basically the same point regarding Israeli advances in irrigation technology.

As is the case with all works, each of these books has its strong and weak points. The major strength of the Nakhleh book is its sensitivity to the needs and perceptions of Gulf residents and rulers. Nakhleh accurately points out the difficulties American policymakers will have if they continue to rely on strictly American views of the Gulf's problems. Beyond this general theme, Nakhleh goes into some depth in discussing politics in Bahrain, the Gulf Cooperation Council, and Saudi Palestinian policy. Unfortunately, he goes into detail about little else of major note. Given the relatively short length of the book, more background on contemporary Gulf politics, perhaps even on a country-by-country basis, would have been helpful. More information on the substantive details of American Gulf policy might have been useful as well, because the examination of that policy in its own right occupies only one chapter of the book.

The strengths and weaknesses of the Allen and Volgyes book are the strengths and weaknesses of each of the book's essays. Many people will find Endelman's claim that the Jews have a "better" claim to Palestine than do Palestinian Arabs unconvincing, and his contention invokes an intransigence on the occupied territories issue that has already seriously impaired the negotiations. Both the Shaked and Leonard essays rally around a Camp David process already discredited in the Arab world. The Perlmutter and Handel essays also endorse the qualitative expansion of an American military policy which Nakhleh, Keddie, Said and Sportiche, and numerous others may see as counterproductive to American policy goals, if not doomed to utter failure. On the other hand, the Rubinstein, Aspaturian, and Gilberg chapters are uniformly strong. Finally, the Gustafson and McKell essays offer instrumentalities that could shed a glimmer of hope at some point in the future. In sum, *Israel, the Middle East, and U.S. Interests* is rich in substantive information yet its analysis demonstrates some bias in the general direction of endorsing the major trends of current American Mideast policy.

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Soviet Strategic Forces: Requirements and Responses. By Robert P. Berman and John C. Baker. (Washington, D.C.: Brookings Institution, 1982. Pp. vii + 171. $22.95, cloth; $8.95, paper.)

This is the sort of book we have come to expect of the Brookings Studies in Defense Policy: solid analysis and a clear point of view based upon a wealth of evidence. It is a book that should be read carefully by thoughtful policymakers and serious students of national security and arms control.

The principal value of Berman and Baker's study is that it views Soviet strategic forces in the context of Soviet doctrines and strategies, providing us with a set of criteria with which to sensibly evaluate Soviet capabilities and, by extension, some useful guidelines to formulate an effective Western response. For this reason, *Soviet Strategic Forces* is a welcome—and needed—contribution to a rich literature of East-West force comparisons, too many of which simply present comparisons of Soviet and Western forces and then attempt to deduce Soviet doctrine and intentions from observations of actual capabilities.

What emerges as an especially critical factor in Soviet thinking is their perceived need to dominate surrounding regions, Europe in particular, in order to defend the Soviet homeland. Consequently, the Soviets stress, probably more than we do, the importance of regional nuclear forces as an element of their view of the strategic balance. This does not necessarily mean, however, that Soviet force capabilities are perfectly tailored to meet Soviet strategic/regional security needs. Indeed, one is not certain whether to be reassured by the authors' convincing description of the organizational biases affecting the development of both doctrine and force capabilities. This is not an entirely new discovery, of course, for Marshal Sokolovskiy's well-known volume, *Soviet Military Strategy* (Crane, Russak, 1975)—no doubt a product of group-think—amply demonstrates the uneasy compromise between Soviet "modernists" and "traditionalists." What Berman and Baker do very well, however, is to provide a succinct yet well-documented account of the sometimes awkward and incongruous organizational and doctrinal shifts that took place in the 1960s and 1970s. The Soviets, it appears, are not really 10 feet tall—perhaps about 6 feet.

That the authors are able to tell this story, including the creation of the Strategic Rocket Forces and the (slow in coming) shift in naval strategy toward a more active strategic strike role, present the usual detailed tables of delivery systems, and put forward some pertinent questions for U.S. policymakers in just 171 pages including footnotes, appendixes, and index is truly remarkable. One of the reasons for their successful presentation is the tight organization of the book, which really is in two parts. The first part consists of four narrative chapters concentrating on the development of doctrine and force capabilities. The second part includes five appendixes on specific technical topics not always encountered, such as the process of missile design, the characteristics of various design bureaus, targeting, and the use of reconnaisance satellites. Each of the two parts could, in fact, stand alone as a worthwhile book; at the same time, each complements the other nicely.

There are no serious weaknesses of *Soviet Strategic Forces*, but there is one major limitation. Like most studies of this sort it will probably soon be dated. As the authors show, the development of Soviet force posture is influenced not only by Soviet doctrine and traditions, but also by Western actions. Recent refinements to U.S. countervailing strategy, attempts to implement extended deterrence, and the development of new weapons systems are certain to be reflected sooner or later in changes in Soviet forces. Let us hope, therefore, that Brookings will keep this excellent study alive and up to date with a future revised edition.

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Afghanistan and the Soviet Union. By Henry Bradsher. (Durham, N.C.: Duke University Press, 1983. Pp. viii + 324. $32.50, cloth; $12.75, paper.)

Henry Bradsher has written an excellent study of past and present relations between Afghanistan and the Soviet Union. His history provides us with a valuable framework in which to more clearly understand both the 1979 Soviet invasion of that nonaligned Asian country and the future of the Soviet role there. Bradsher's journalistic skills and personal familiarity with the countries involved are amply displayed in this well-written book. Moreover, his scholarly approach is also quite evident: The book has been broadly researched, carefully argued, and documented in detail. His numerous sources (more than one-half of the chapters contain more than 100 notes) range from interviews, countless newspapers and radio broadcasts, and documents to books and articles. There is a most useful bibliography of some 15 pages.

Among the many contributions of this book is its focus on the depth of the Soviet Union's involvement in Afghanistan over the decades before the 1970s. The early chapters are devoted to a discussion of internal events in Afghanistan and the growing Soviet efforts to influence these developments from 1917 to the 1973 coup that overthrew King Zahir Shah. The next two chapters deal with the Daoud period that followed and the 1978 communist coup that overthrew him. The next four chapters concentrate on the immediate antecedents to Moscow's invasion in 1979 and the invasion itself. The final three focus on the consequences of the invasion and the current and future nature of the Soviet-Afghan relationship. Bradsher presents persuasive evidence of the growing Soviet stake in Afghanistan, a stake the Kremlin came to see threatened by the events of 1979.

A second particular value of Bradsher's analysis is his evaluation of the Soviet leadership's motivations for undertaking the invasion. He ably discusses internal and external factors in Soviet decision making and even speculates, on the basis of admittedly limited data, who in the Kremlin was likely involved in the process. He examines

Soviet leaders' perceptions of the USSR's security interests, its ideological stake in the country, military concerns, the psychological stake of standing by a leftist government, and the possibility of the offensive motivation of getting closer to warm water ports and Gulf oil. His conclusion of the "offensive versus defensive" debate among Western scholars is that the distinction between the two is blurred: "in the short term" the invasion "had a defensive quality; in the long term it offered offensive possibilities even if they were not part of the original calculation" (p. 160). in both chapters 7 and 8, "The Correlation of Forces" and "The View from the Kremlin," Bradsher analyzes these factors as well as the Soviet military buildup, recent Soviet actions in the Third World and the U.S. response, and the evolving relationship with Washington.

A third significant contribution is Bradsher's discussion of the future. He assesses, particularly in his concluding chapter rather pessimistically titled "Soviet Afghanistan," the likelihood of a negotiated solution to the situation, a solution which would feature a Soviet withdrawal. Bradsher discusses a wide range of factors that provide little hope of such an outcome. These include Afghanistan's geographical position between the USSR and an unstable region, the involvement of Soviet prestige, the strong ties between the Soviet Community party and the Afghan one (PDPA), and the disinclination to admit a mistake. Here and in other places in the study Bradsher also examines other examples of Soviet intervention for parallels as well as clues to the future. The most similar and predictive cases in his analysis are the establishment of Soviet power in Central Asia and the converting of Mongolia to satellite status, both accomplished primarily in the 1920s. On the basis of his evidence and his discussion of these analogies, Bradsher's perception of the future of the Soviet Union in Afghanistan, unfortunately for the Afghan people and other supporters of freedom and independence, seems not so much pessimistic as discouragingly realistic.

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People, States and Fear: The National Security Problem in International Relations. By Barry Buzan. (Chapel Hill, N.C.: University of North Carolina Press, 1983. Pp. x + 262. $24.95, cloth; $8.95, paper.)

The national security problem in international relations that this book addresses is a conceptual or a contextual one. Both the practical pursuit and the scholarly analysis of national security, it argues, have so focused on rapidly changing technologies and short-term demands that they have given no serious thought to the nature of the objective that is being pursued, and have thus provided no really worthwhile appraisal of the means or the strategies of pursuing it. The carefully limited purpose of this book is "not to seek solutions to particular [security] policy problems, but to alter, and hopefully to expand and enrich, the background of ideas against which [such] particular policy problems are viewed" (p. 248). That purpose is achieved.

The argument offered is highly organized, intricately developed, abstract, and normative. It ends by advocating a "holistic" view of security that will incorporate the individual, national, and international dimensions of the concepts that have been identified and explored earlier into a new brand of "systemic security" (p. 247) that can be best realized by a "multi-layered approach" (p. 254).

There are at least two problems. One is vagueness of prescription or, as often seems to be the case, of merely alternative nomenclatures. For example, systemic security "carries the requisite sense of parts, and the relationship among them" and "directs our attention to the integration of the three levels which we have argued is essential for understanding contradictions which define the problem . . ." (p. 248). In other words, look at the big picture, and see it whole—a recommendation that may be as difficult to implement as it is to dispute.

The other problem is the highly questionable likelihood of any practical realization of Buzan's advice. On this, he is everywhere completely disarming: One desideratum, "improved anarchies," although "not so far off as world government," is "by no means within each reach" (p. 123); he admits that his whole analytical scheme "begs serious questions from the real world" (p. 254). It is hard to contend with an analyst who is so forthcoming about the realistic limitations of what he is proposing. But the limitations are not thereby removed.

There are other, lesser, irritations. For example, why is David Singer ignored almost entirely in an area of analysis where his own contributions have been not only substantial but even seminal? One looks in vain for reference to Singer's 1961 "levels-of-analysis problem" article or to the elaborate and now long-standing Correlates of War project at the University of Michigan, both of which would seem highly germane to important parts of Buzan's argument.

Why are reliable measurements of what are otherwise mysteriously impressionistic classifications so assiduously avoided? For example, the entries in Buzan's Figure 5.1 (p. 132) could have been easily documented by reference to readily available empirical findings, but they are not. Buzan, to be sure, insists that he is not attempting an empirical analysis. But appropriate empirical evidence could have quieted the fears of some readers that scientific rigor might be being sacrificed to the symmetry of pleasantly orderly abstractions.

Still, this book should be read. It should be read by three groups of analysts in particular: by discouraged liberal internationalists who may be modestly heartened; by the old-style realists who may need to ponder the implications that Buzan delineates of the mode of realist thinking itself; and, before it is too late, by the very members of the national security profession, practitioners and scholars alike, to whom this analysis is primarily addressed.

This is a rich, wise, and thought-provoking book.

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The New Nationalism and the Use of Common Spaces: Issues in Marine Pollution and the Exploitation of Antarctica. Edited by Jonathan I. Charney. (Totowa, N.J.: Allanheld, Osmun, 1982. Pp. 342. $39.50.)

This collection of original essays represents one of the most laudable scholarly efforts to date aimed at examining the resource potential of, and environmental threats to, ocean space and Antarctica. Published under the auspices of the American Society of International Law, this compendium takes a no-nonsense look at the ominous evidence of man-made marine pollution, as well as the realistic portent that intense nationalistic pressures and rivalries for natural resources might also precipitate widespread environmental degradation of the Antarctic ecosystem. To the editor's and authors' combined credit, the composite product emerges as politically provocative, legally sophisticated, and intellectually sound.

Structurally, this volume consists of nine essays that are divided into two major sections. The first section deals with marine pollution in general, whereas the second concerns the Antarctic region specifically. The initial contribution by Jan Schneider authoritatively assesses the international legal instruments presently available for deterring pollution from oceangoing vessels. William Whipple, Jr. cogently evaluates the sources of land-based pollutants (for example, metallic effluents, petroleum, chlorinated hydrocarbons, radioactive substances, and sewage runoff), particularly as they are regulated by established national controls. The political and economic dimensions of land-based pollution sources are thereafter thoughtfully appraised by Judith Kildow, leaving the legal facets of the problem to be addressed by Robert J. McManus. Given the recognized threat of global marine pollution, his conclusion is hardly reassuring:

> It cannot be doubted that a workable international scheme for the abatement of land-based sources of marine pollution could be adopted and implemented only after formidable political obstacles had been overcome. Moreover, the will to do so . . . is unlikely to exist without far more persuasive evidence of an incipient marine ecological collapse than now exists. (p. 107)

The second part of the book undertakes to analyze the management of Antarctica's resources and the imperative to construct effective legal regimes for their exploitation should that ever occur. The five contributions are exceptionally incisive, perspicacious, and replete with valuable information. James H. Zumberge supplies a wide-ranging analysis of Antarctica's natural resource potential and the serious ecological problems likely to occur in the wake of its precipitous exploitation. As he so clearly points out, the grave environmental risks associated with possible offshore hydrocarbon production loom particularly large and therefore merit all the more especial concern. The next essay by Giulio Pontecorvo disparages the economic attractiveness of Antarctica's resources, at least for several more decades. The extravagant costs of exploitation there, coupled with tremendous logistical difficulties and assured adequate supplies from traditional sources, simply far outweigh market demands for exploiting Antarctica's mineral resource base in the foreseeable future. In an admirably balanced and comprehensive treatment, Richard Bilder critically examines the contemporary legal and political situation in Antarctica. The thrust of his analysis explores the issue of mineral resource exploitation vis-à-vis the jurisdictional scope of the Antarctic Treaty and the conflict of claimed sovereignties in the region. Charney's own thoughts on these issues are contained in his contribution, "Future Strategies for an Antarctic Mineral Resource Regime–Can the Environment Be Protected?" His answer to the question leans towards the affirmative, provided that incremental efforts are successful in fashioning "a limited regime to manage those exploration activities likely to take place within the next 20 years, the exploration of hydrocarbons on the Antarctic continental shelf" (pp. 230-231).

The final essay, by James N. Barnes, reveals the course of negotiations from 1975 to 1979, which culminated in 1980 in the Antarctic Treaty Consultative Parties promulgating their Convention on the Conservation of Antarctic Marine Living Resources. Although "logically developed and comprehensive," Barnes faults the Convention for its consensus decision-making structure and its relatively weak enforcement procedure. The Convention is inadequate, he concludes. In its stead is needed "a rational and equitable mechanism for *managing* activities on the continent and in the surrounding seas in order to protect interests and resources of importance to the entire international community" (p. 269).

The book carries an index and includes as appendixes two maps of Antarctica and four relevant international documents. *The New Nationalism and the Use of Common Spaces* is a timely and welcome scholarly contribution to the international relations literature and is especially highly recommended for students and researchers interested in international environmental law.

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The ASEAN States: Coping with Dependence. By Donald K. Crone. (New York: Praeger Publishers, 1983. Pp. x + 230. $25.95.)

Donald Crone presents an interesting thesis: Developing countries can control and reduce dependence on the major industrialized powers by diversification of trade and investment and by regional cooperation. But the empirical data collected in support of his thesis are, to me, its most valuable part. The book is not explicitly concerned with an attempt to prove that dependence has either positive or negative effects; nor does it discuss the interaction between dependent countries and would-be dominant powers. It is concerned not with the condition of dependence, but with policies designed to "cope with" dependence.

Crone compares three schools of international political economy: advocates of economic nationalism, dependency theorists, and those of interdependence. He claims to combine the insights of all three in a "composite strategy," but his inquiry into trade and investment diversification relies on concepts of sensitivity and vulnerability drawn from interdependence studies. Sensitivity denotes the extent to which impulses from the international system are transmitted to domestic economies. On the other hand, vulnerability refers to the degree to which economic relations are concentrated with any particular country. "For diversification to be an effective strategy to counter dependence, both sensitivity and vulnerability should be lessened" (p. 28).

Crone's decision to study what I would call the tactics of diversification rather than the strategy of dependence is not just a matter of his right to fix the bounds of his work, for his decision has important implications. How, for example, can he say that countering dependence is the issue if he does not seriously discuss what dependence is, in the first place? From his arguments on page 28—and his conclusion on page 185 when he rejects "delinking" as unrealistic—he would appear to accept the interdependence thesis of mutual, if unequal, benefit: The country with stronger bargaining power gets more out of the relationship than the weaker, but both gain. If so, the benefit of integration within the capitalist world market (contrary to the dependency thesis) is the basic consideration; sensitivity and vulnerability are accordingly lesser issues and in any case contingent on the former. Nevertheless, Crone does not attempt to prove the mutual benefit argument; it's just taken for granted.

If, however, Crone had opted for the dependency thesis, then "mutual benefit" is seriously undermined. Any benefits are far outweighed by the costs: a peripheral economy distorted by the requirements of the center and thus incapable of autonomous growth, and the reinforcement of the power of a clientel elite whose function is to serve external interests. These are formidable disadvantages. But, again, Crone is not concerned to prove or disprove that thesis.

Thus, whether it is a matter of dependence or interdependence, to limit the inquiry to the impact of diversification on sensitivity and vulnerability is to miss the main target. But even the lesser issues are assumed rather than explained. For Crone does not pursue the question, What precisely is the effect on the "local" economy and society of being a vulnerable partner in a world economy dominated by big powers? One reason for the omission is methodological. Crone does not discuss the role of trade or investment in the overall context of the economy of the countries he is studying (members of ASEAN). He considers the segment—for example, trade—in isolation, and then decides how to cope with potential disadvantages. Such a form of abstraction, which is common among social scientists, arises paradoxically from system-orientation: Concern with the system conditions the approach to the "parts" of the system.

To my mind, an analysis of the "structural pattern of power" requires a wider contextual inquiry—that is, into the role of the parts in the system, and the specific effect of the system on any of the parts. It is not enough, in line with scientific doctrine, merely to collect quantifiable data. Yet, as I have acknowledged, this collection is by no means without merit. Indeed, the details of regional organization, revealing an increase in bargaining power, are enlightening.

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Managing International Crises. Edited by Daniel Frei. (Beverly Hills, Calif.: Sage Publications, 1982. Pp. 240. $25.00.)

This collection of original papers is the product of a round table held in 1981 at the University of Zurich under the auspices of the International Political Science Association. Because they are highly visible and often dangerous confrontations, international crises have attracted increasing attention from both scholars and practicioners, and this is reflected in the contributions to this volume. The motivation is self-evident: a pervasive fear of uncontrolled warfare, particularly the possibility of an unwanted nuclear war. This collection brings together a comprehensive, authoritative, and eclectic survey of theorization, research, and practice dealing with crises as salient international events.

The majority of international crises—variously defined—since the 1950s have been between secondary rather than nuclear powers. The frequency of crisis episodes has risen fourfold in the last two decades. In fact, crisis politics has become a major style of international interaction. Although the role of force as a feature of crisis politics has diminished in some respects, the use of force has become more sophisticated and fine-tuned. The growing frequency and volatility of international crises has magnified the critical need for prompt and well-informed communication among global actors, a need that is severely taxed by the multiplication in numbers of global actors. Hence, communication is a recurring theme in these papers as are decision making, strategy, and institutional development.

Karl Deutsch states the problem as a classic decision situation involving the management of information. Decision makers in a crisis must act on the basis of limited information, reaching decisions through limited channels within a limited time, contaminated by noise, and affected by desires, fears, rage, and/or misrepresentations put forth by adversary interests. The more intense and acute the crisis, and the more limited the information search and processing capacities of the decision-making system, the more decisions are likely to be wrong. Deutsch's analysis also attends to problems of content meaning and decision-maker memory. The communication perspective is further developed by John Meisel, who explores the way in which print and electronic media interpret and affect international crises.

Several contributors concern themselves with empirical data and issues pertinent to crisis management. Jean Laponce reviews a number of well-known *in vivo* and laboratory experiments and is optimistic about the fruitfulness of simulation findings for practical work in international and other fields. Vadim Lukov and Victor Sergeev, examining the structure and dynamics of perception of crises and applying a cognitive-mapping approach, draw their data from the divergent behaviors of principal German foreign policymakers—Bismarck, Bülow, Bethmann-Hollweg, and Wilhelm II—from 1866 to 1914. Ali E. Hillal Dessouki tests the applicability of numerous familiar crisis management propositions to the Middle East crises of 1967 and 1973, particularly as these propositions deal with perceptions of threat, communication with a superpower ally, the degree to which options seem limited, and the dangers of relying upon past experience.

Several contributors are preoccupied with improving conceptualizations of international crises. Richard Merritt stresses the value of a probabilistic approach that notices the regularity with which certain international problems arise and therefore enable decision makers to develop standard operating procedures with which to respond to problems. Hayward Alker and Frank Sherman compare several analytical approaches to the study of crises and wars, focusing on problems of operationalizing such key terms as order, security, crisis, aggression, peace, and war.

Institutional issues are treated by three authors. Kinhide Mushakoji observes that a new conflict management role must be developed for the United Nations as the capacity of the superpowers to maintain stability and security declines. Radovan Vukadinović sees a need for better instruments of conflict resolution among nonaligned countries, particularly as bipolar tensions relax. The détente achieved by the Conference on Security and Cooperation in Europe is, according to Karl Möttölä, a substantial model for the evolving global system despite the difficulties of détente generally and the military balance specifically. All three authors express a sense of urgency regarding the need for institutional development for the purposes of crisis management.

Finally, there are the practical aspects of international crises. Daniel Frei looks at the danger of unintended nuclear war and concludes that a greater risk arises from strategic instability than from accidents. King-Yuh Chang derives from the literature a host of desirable diplomatic behaviors designed to cope peacefully with crises. The book concludes with brief comments from two practicing diplomats: Robert de O. Campos (Brazil) and Niels Hansen (Federal Republic of Germany).

With one or two exceptions, the book is impressively readable, and the authors employ illustrative materials that require only modest background on the part of the reader. Collections of scholarly papers do not often lend themselves to classroom use, but this one does. The volume is also likely to help crisis analysts in different cultures to orient themselves more readily to the state of knowledge in the crisis management field.

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Environment, Development and Military Activity: Towards Alternative Security Doctrines. By Johan Galtung. (Oslo: Universitatsforlaget, 1982. Pp. 143. $15.00, paper.)

Johan Galtung wrote this book while working as a special consultant for the UNEP Regional Office for Europe, assisting in the preparation for the 1982 Second Special Session on Disarmament of the General Assembly of the United Nations. The work is remarkably macrocosmic in its approach, and its principal purpose is to show the link between issues of ecology and those of national security.

More specifically, the book analyzes the relationship among the environment, composed of the cosmosphere, atmosphere, hydrosphere, lithosphere, biosphere, and homosphere; development, broken down into individual human development and social development; and military activity, including military preparation and military action. The central impact of the military upon the environment involves pollution, depletion, and contamination (pp. 35-41), and that on development involves the opportunity costs in terms of basic needs not realized (p. 41). A number of propositions emerge about the linkages among these areas: for example, that military action (particularly using nuclear and "radiological/geophysical" weapons) is more destructive than military preparation (p. 58); that the homosphere is the hardest hit (p. 58); and that the cycles and feedback loops involved in the relationship of war to environmental and social degradation make the negative effects involved more widespread, long-lasting, and severe (pp. 61, 66-71).

To cope with these pernicious consequences, Galtung suggests a number of "security doctrines." The definition used of security is that a society is essentially stable and has any basic change coming from the society's "internal dialectic" (p. 75). He rejects conventional approaches to security involving offensive military capability because (p. 81) their potential destruction level is too high, they "will destroy what they are supposed to deliver," and they lead to arms races. Instead Galtung advocates a number of alternative approaches: at the national and local levels, a state should pursue a stable ecosystem through combining self-sufficiency in essentials with equitable exchange for other goods and services (pp. 83-85); at the intra-national or societal level, a state should promote invulnerability by "overcoming cleavages caused by injustice, inequality, and inequity" (pp. 84-85); and at the individual level, a state should foster "a way of life that is satisfactory" (p. 85). In each case, "invulnerability is intrinsically tied to environmental principles, not only in the sense that the stability of the eco-system enters as a key necessary condition, but also in the sense that there is much to learn from the built-in wisdom of a mature ecosystem" (p. 89). Galtung further argues that aiding the security of others increases one's own security because "security is indivisible" (p. 97). Finally, he advocates changing paradigms from armament-disarmament thinking to "transarmament" policies, which can be undertaken by nations acting alone and focusing on eliminating offensive weapons and on "building invulnerability" (pp. 102-109).

Although provocative, the primary flaw of this work is that its generalities gloss over major controversies. For example, there are significant debates over whether, and under what conditions, offensive weapons can be definitionally separated from defensive weapons; security is truly indivisible; natural systems or "ecological reasoning" are necessarily optimal; and self-sufficiency in essentials is desirable for developed nations. This book neither qualifies nor extensively defends these contentions. A related problem is that Galtung neglects the major tradeoffs between national security and environmental protection and seems to assume that the two concerns are almost completely harmonious. Finally, the prescriptions contain virtually no treatment of feasibility problems, drastically reducing the book's practical policy relevance. The writing style is repeatedly emotional and sometimes melodramatic (citing nuclear destruction as worse than the medieval plague or the Biblical flood), and that too inhibits effective analysis.

In sum, although Galtung outlines some fruitful conceptual linkages between military and environmental issues, they are often oversimplified, inadequately qualified, and unrealistic. A more detailed and skeptical appraisal of these concerns is clearly in order.

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Strategic Studies and Public Policy: The American Experience. By Colin S. Gray. (Lexington: University Press of Kentucky, 1982. Pp. 1 + 230. $19.50.)

Colin Gray is among the most prolific and provocative strategists writing today. His publications place him along the conservative end of the spectrum on defense matters. In this book he attempts to suppress his "strong opinions" (p. 1) to "record and interpret" (p. 1) the evolution of strategic studies. For the most part, he has produced a competent summary of the discipline's development that will be of particular interest to the uninitiated. Professionals will be familiar with much of the ground Gray covers, although even they may learn matters of value by reading this study. At the same time, regrettably, the work is marred by cheap shots at those, notably arms controllers, who do not share Gray's intellectual predilection.

The book begins with a useful definitional survey and summary of catalysts for strategic inquiry including agencies that make and execute policy, military technological innovation, scholarly curiosity, and dissatisfaction and personal career motivation. Following a review of the nexus between theory and strategy, Gray begins his historical recapitulation. He distinguishes three periods of analytical ferment: 1945 to 1946, 1955 to 1965, and 1969 to the present. The first epoch entailed the initial flush of intellectual endeavor as Bernard Brodie and his contemporaries tried to make sense out of the nuclear age for defense planning. Following a lacuna in the influence of professional strategists through Eisenhower's adoption of massive retaliation (about which Gray presents challenging insights contrary to conventional wisdom), a strategic "golden age" (p. 45) emerged. Through the mid-1950s and into the 1960s the foundation of modern nuclear strategy was laid through intellectual pulling and hauling. Gray describes how the United States moved away from massive retaliation toward strategies of counterforce, assured destruction and damage limitation, limited and local war (including a review of Vietnam), and how in later years the Carter administration came to adopt Presidential Directive 59, which attempts to deter Soviet aggression by focusing on assets of value to the Russian leadership.

Throughout the analysis Gray provides useful admonitions. He critiques American policymakers for assuming that the Soviets are doctrinally deficient because they do not behave as we do in their strategic deployment. He feels that we should appreciate the unique perspectives of our adversaries. Such advice certainly deserves a fair hearing. By contrast, Gray's *ad hominem* remarks about arms controllers are more questionable. Such individuals, defined as extremists, are held responsible for today's alleged American strategic inferiority. Gray asserts that this inferiority is due to the intellectual deficiencies of the arms controllers. For example, concerning the SALT II debate, Gray writes: "It is probably true that the quality of analytical talent on the conservative side was markedly superior to that on the liberal side" (p. 165). This pithiness extends to Gray's evaluation to America's performance in Vietnam. Failure is laid in part to "strictly second-echelon strategic thinkers (with Walt Rostow as their Pied Piper) [who] were permitted to propagate ill-considered theories on counterinsurgency and nation-building virtually without professional challenge" (pp. 121-122). In this regard, Gray's critique may be somewhat disingenuous. After all, he himself finds little value in arms controllers who critique his predispositions, and even less value for those who espouse more unorthodox views, such as world-order modelers who engage in "agreeable playthings for an idle hour" but who "can have no respected place in an aspiring discipline—international relations—which has, from its inception, been conceived to be policy-relevant" (p. 190). His solution is to understand international politics in terms of "national interest" and "balance of power" (p. 190). But without operational definitions (e.g., what is the national interest in a pluralist society as the United States? what is the utility of the balance of power concept for which Ernst Haas has identified eight different definitions?) such guidance is imperfect.

In sum, although the book has its share of good scholarship, it is also colored by Gray's professional bias, of which the reader should be aware.

BENNETT RAMBERG

University of California, Los Angeles

The Forgotten Victim: A History of the Civilian. By Richard Shelly Hartigan. (Chicago: Precedent Publishing, 1982. Pp. xi + 173. $17.95.)

The distinction between soldier and noncombatant has long constituted one measure of a civilized polity according to this historical examination of the theory and practice of civilian immunity. Hartigan's thesis is that although civilian immunity from warfare evolved as an adaptation by the human species for survival, this immunity has been negated by nuclear deterrence strategies that raise "military necessity to an end in itself" (p. 9).

This work is a succinct and sobering chronology of Christian and secular just war practices and a lively elucidation of the expansion of international standards of noncombatant protection during and after the Enlightenment. Hartigan begins by considering how enslavement of the vanquished in ancient times sprang from recognition of their economic value. Noting that this practice established the precedent of characterizing the defeated as morally inferior to the victors, Hartigan suggests that Aristotle's distinction between Greek and barbarian (p. 20) later found its way into Medieval doctrines that differentiated proper treatment for Christians and non-Christians.

Later philosophical controversies centered around justifications for self-defense and killing in the name of public authority. Departing from Augustine's notion of self-defense as unjustifiable and war as appropriate only as a means to establish a "renewed moral order" (p. 26), Aquinas' doctrine of proportionality, which championed moderation in efforts to resist death and labelled publicly endorsed killing as morally nonresponsible, endorsed positions that remain problematic today (p. 48). Later chapters, devoted to efforts by church councils to protect the civilian from dynastic rivalries and by philosophers, jurists, and tribunals to extend noncombatant protections (such as Francis Lieber's codification of battlefield conduct during the American Civil War), reveal an appreciation of the relationship between international law and political philosophy.

There are three serious deficiencies in this otherwise notable effort to trace a theory of civilian immunity in Western political thought. First, the book's conclusion, a polemic on the arms race, expresses sentiments articulated by the United States' Catholic bishops' pastoral letter on nuclear warfare but fails to examine the deeper flaws in nuclear deterrence. Hartigan contends that because nuclear weapons cannot discriminate between civilian and military targets, civilian immunity is considered fatuous by military leaders, a facile claim on Hartigan's part in light of the continuing search for credible strategies designed to halt total holocaust through scaled escalatory policies and precisely targetable weapons. Although such tactics may prove to be futile, they underscore a growing recognition that current deterrence doctrines are fratricidal.

Second, Hartigan omits consideration of many nonreligious influences upon civilian immunity doctrines. For example, although he discusses at some length Francisco de Vitoria, a fifteenth-century monk who advocated categorically sparing all nondangerous, defeated populations (p. 86), he ignores Machiavelli's contributions to such a doctrine (*Discourses*, book 3, chap. XII, 530), especially Machiavelli's directives toward using mercy to court favor with the vanquished. Similarly, Hobbes's notions of kingly obligation to defend subjects from war, and subjects' rights (in the absence of authority) to defend themselves with appropriate force is not cited (*Leviathan*, part II, chap. 29).

Third, Hartigan claims that civilian immunity was expanded to more ethnic groups after Grotius' time (p. 102), but ignores regressive tendencies including the genocide of Native Americans. In this same vein, although acknowledging the effect of the airplane and guerilla warfare upon civilian immunity before the advent of nuclear weapons, Hartigan does not discuss how the submarine jeopardized practices designed to balance freedom of the seas with the military necessity for combat zone arms embargoes.

In conclusion, although *The Forgotten Victim* is a significant review of the history of civilian immunity, it falls short of suggesting how to reinstate such a doctrine. It is hoped that Hartigan will explore this issue in another volume.

DAVID LEWIS FELDMAN

Moorhead State University

Olympic Sports and Propaganda Games: Moscow 1980. By Baruch A. Hazan. (New Brunswick, N.J.: Transaction Books, 1982. Pp. 218. $19.95.)

The Olympic games began as sacred festivals but today inspire adoration of secular states and societies that produce medalists in large quantities. Because the games impart glory to national winners, it is not surprising that athletes increasingly are seen as political resources and the games as political, not simply athletic, events.

Baruch Hazan argues that the 1980 Moscow Olympic games "served as conclusive proof that sport and politics are connected, and that sport in fact *is* politics, that major international competitions are major business enterprises, and that all top world athletes . . . are professionals" (p. 203). He reaches this conclusion after a close examination of "the propaganda aspect of Soviet sport in general and the 1980 Moscow Olympic Games in particular" (p. 6).

Political scientists concerned with socialization, propaganda and public opinion, and international relations should all find occasional gems in this study.

For example, Hazan demonstrates that a Soviet athletic trainer's life is not an easy one, that ideological virtue as well as conventional physical prowess is stressed. When the going gets tough for individualistic Americans, the tough supposedly get going, but for the Russians (quoting a 1980 *Sovetskiy Sport* editorial): "When the going gets tough our sportsmen remember the Motherland, which gives them strong wings for an eagle's flight" (p. 39). Americans, to be sure, also are reminded to do their national duty but in less emphatic terms.

The specialist in propaganda will find numerous illustrations vividly portraying the Soviet attempt to win friends through athletic success and by hosting the 1980 Olympics. Thus, a special soft drink named Drushba (friendship) was introduced in honor of the games in addition to other more familiar promotional devices. But despite Drushba, a massive publicity campaign, and a staggering collection of gold medals, Soviet objectives only partially were realized, to some extent because of the fraudulent way in which some victories were obtained; for example, the entire Romanian press exploded with rage when their national heroine and past triple Olympic champion, Nadia Comaneci, was supplanted by a Soviet rival due to unfair judging.

Specialists in international affairs may find Hazan's analysis of American-Soviet jousting over the Carter-sponsored boycott protesting the Soviet invasion of Afghanistan valuable, and glimpses into the uneasy Soviet relations with its satellites in Eastern Europe intriguing.

Hazan is less successful when he links sports to politics—he makes the obvious theoretical connections but shows no familiarity with recent work in sports sociology that would have provided a richer perspective.

He also assumes too sharp a distinction between bad and good guys, as when he asserts: "Democratic regimes, unable to control mass media . . . are seldom a match for authoritarian states' propaganda apparatus" (p. 9). Yet the propaganda winners during World War I clearly seem to have been the democratic American and British governments, and the American media, although hardly subservient to the state, is not entirely out of control, either. Even the *New York Times*, after all, admits that it hasn't always published "all the news that's fit to print" when political leaders have intervened.

Hazan's enthusiasm for the free world leads him to assert that U.S. groups opposing the boycott "played straight into the hands of the Soviet propaganda machine" turning them "into instruments of Soviet propaganda" (p. 154). These and other words on the same subject suggest the very "sports commissar" mentality he elsewhere implicitly condemns.

GENE WEINSTEIN

San Francisco State

Sino-American Normalization and Its Policy Implications. Edited by Gene T. Hsiao and Michael Witunski. (New York: Praeger Publishers, 1983. Pp. vii + 515. $45.00, cloth; $15.95, paper.)

Gene T. Hsiao and Michael Witunski have attempted a herculean task: the covering of all important aspects of United States-People's Republic of China relations from the 1979 exchange of ambassadors through the first half of the Reagan administration. Not surprisingly, the results of this effort are mixed. The six chapters in this thick volume, written by four different scholars, present an uneven glance at those early years of full diplomatic ties. Extensive documentary appendixes (covering some 300 pages) supplement this book's substantive discussion.

All of the chapters in *Sino-American Normalization* are well-crafted, yet those who have closely followed events in Asia will find little here in the way of new information. For example, Charles W. Freeman, Jr. presents a largely chronological account of the forging of diplomatic ties between the United States and the People's Republic of China and of the occurrences that followed during the immediate postnormalization period. Although his account is interesting, it does not add to our understanding of the dynamics that stand behind America's China ties. This same analytical defect scars Pierre M. Perrolle's otherwise fine chapter on educational exchanges between the Mainland and the United States.

The highlights of this volume are Hsiao's two chapter on Taiwan and William R. Feeney's dissection of the strategic bonds that presently "glue" Washington to Peking. Again, these chapters do not present much in the way of novel information. It is their interpretations, at times controversial, that provide their worth.

Hsiao begins by discussing the legal aspects of the Taiwan question. He finds the legalistic base of America's policy toward Taipei to be at variance with its rhetorical foundation. This tension arises out of the Taiwan Relations Act (TRA) which, in Hsiao's eyes, has "rendered inoperative" America's longstanding rhetorical commitment to a unified China. "Indeed, it is a supreme irony in Sino-American relations that after nearly three decades of bitter argument between Beijing, Taipei, and Washington, the 'two China' policy has finally become a legal reality under a single instrument of US domestic law . . ." (p. 79). Hsiao's chapter is strongest in arguing this point, although weakest in considering whether political rhetoric in international relations is of more substance than legal "reality."

The second portion of Hsiao's discussion deserves even wider attention. In this section, he addresses the future viability of Taiwan, and of America's China policy, in light of events since the passage of the TRA. He concludes that an American "policy based on permanent ambiguity (toward Taiwan) cannot possibly serve any national interest of the United States in the long run" (p. 120). Nor would this policy benefit the people living on Taiwan because it is likely to foster their illusions of "going it alone," a course which Hsiao sees as a prescription for disaster leading to the forced reunification of Taiwan with the Mainland. Events are moving much faster in this area of the world than most Americans understand. According to Hsiao, the day of reckoning for Taiwan—and, therefore, for America's Far Eastern strategy—is much closer than most choose to believe.

The value and durability of America's strategic tie to China is the focus of Feeney's contribution. His is a first-rate discussion of the military, economic, and geopolitical factors that will in the future effect America's "alliance" with Peking. On balance, Feeney believes it likely that strategic ties between the two countries, tentative though they may be, will endure due to a shared antipathy toward the Soviet Union. This outcome, however, is not foreordained. It is also possible that if relations between Washington and Moscow become even more strained than they already are, Peking will choose to position itself "as the pivot point" (p. 183) of the triangular relationship, thus reaping benefits from both sides. As Feeney implies, the nature of U.S.-PRC-USSR ties will go a long way toward determining the overall shape of world politics for the foreseeable future. It is, therefore, prudent for decision makers to consider all permutations of this relationship in developing America's foreign policy.

LEONARD A. KUSNITZ

Buenos Aires

The Soviet View of U.S. Strategic Doctrine: Implications for Decision Making. By Jonathan Samuel Lockwood. (New Brunswick, N.J.: Transaction Books, 1983. Pp. ix + 202. $19.95.)

The Soviet View of U.S. Strategic Doctrine is a most intriguing examination of Soviet perceptions and reactions to the development of American strategic doctrines since 1954 and, perhaps of most importance, of how these Soviet perceptions have influenced Moscow's strategic policies, development programs, attitudes toward arms control, and foreign policy objectives.

Drawing upon the work of Fritz Ermarth ("Contrasts in American and Soviet Strategic Thought," *International Security*, 1978, *3*, 138-153), Lockwood describes three schools of strategic thinking: the mirror-image school, that characterized by the historical continuity argument, and the third school of thought—as outlined by Ermarth and upon which Lockwood bases his methodology—that the Soviets mean substantially what they say (p. 3).

Lockwood cautions unwary Western analysts by asserting that it is absolutely essential to have a means for distinguishing what the "real" Soviet perceptions of United States strategic doctrine are from propagandistic pronouncements. The real Soviet view of American strategic doctrine, according to Lockwood, is influenced by two primary factors. First, "Soviet analysts project their own strategic mindset onto US strategic planners and interpret the development of US strategic doctrine within the context of that mindset" (p. 4). Second, "ideological interpretation of US intentions as viewed through the ideological prism of Marxism-Leninism predetermines the logical course of US strategic doctrine as perceived by the Soviets to a great extent" (p. 4). Therefore, Soviet statements intended for propagandistic reasons—what Lockwood terms declaratory deterrence—can be identified largely by their intended purpose: "to discredit or otherwise reduce the credibility of US strategic doctrine in terms of its deterrent values against Soviet freedom of action to employ her military power in support of her foreign policy goals" (p. 4).

In covering the development of U.S. strategic doctrines since 1954, Lockwood provides a doctrinal overview of the American rationale for, among other things, the concepts of limited war, graduated deterrence, damage limitation, counterforce, limited nuclear options, and Presidential Directive 59. Likewise, Lockwood covers the massive retaliation period of U.S. strategic doctrine (1954 to 1960), the flexible response period (1961 to 1968), and the period of U.S. doctrine known as realistic deterrence (1969 to 1982).

Throughout the book Lockwood very nicely provides analyses of Soviet perceptions of American strategic doctrine by dividing his investigation into, first, discussions of declaratory deterrence, or the propaganda line put forth in Soviet writings, and, second, discussions of the real Soviet viewpoints.

Given the themes examined by Lockwood, specifically what is the Soviet strategic mindset and what are its implications, if any, for American strategic doctrinal development and decision making? First, Soviet analysts, with an obvious proclivity for strategic mirror-imaging, tend to view American strategic doctrine largely within the framework of their own strategic doctrine. Second, the "Soviet predilection for mirror-imaging came into conflict with the second major theme of Soviet perception that emerged near the end of the massive retaliation period. This was that the Soviets increasingly began to admit, both in public as well as in private analyses, the likelihood and the necessity of having to wage limited wars with the use of conventional and/or nuclear weapons" (p. 173). The Soviets resolved the discrepancy between the themes by concluding "that the evolution of US strategic doctrine was the result of a *forced* response to the growth of Soviet strategic power" (p. 174).

Considering Soviet strategic perceptions and geopolitical motivations, how must the U.S. leadership, as well as its strategic planners, respond to the challenges posed by the Soviet Union? Because Lockwood feels that the main threat to the security and well-being of the U.S. is the possibility of a nuclear war with the USSR, there exists two means of neutralizing this threat. One is the avoidance of nuclear war and the thorough pursuit of arms control and disarmament negotiations with the Soviets. The other is the attainment of a sufficient capability to defeat a Soviet nuclear attack and, at very least, to minimize the effectiveness of any such attack (p. 180). Lockwood puts forth a three-tiered strategic defense that would incorporate a strong civil defense, an antiaircraft and antiballistic missile defense, and a space-based defense system. Lastly, the fulfillment of American grand strategic objectives will depend upon one additional component which "would nullify the military effectiveness of nuclear weapons to the greatest extent possible, and hence negate their political utility as a coercive instrument" (p. 182). According to Lockwood, this component is American ideology.

Lockwood's work is an interesting and innovative approach to Soviet-American strategic relations but, unfortunately, suffers from an obvious sense of moral ethnocentricity and glaring sociopolitical and military biases. Lockwood's investigation would have profited from a more thorough discussion of Soviet conceptions of deterrence (*sderzhivanie* and *ustrashenie*) and an overview of Soviet perceptions of arms control which, after all, is a crucial part of U.S. doctrine and grand strategy. Last, the inclusion of a subject index would have complimented the excellent name index.

Reservations aside, *The Soviet View of US Strategic Doctrine* is an important contribution that should be welcomed by students and specialists interested in Soviet and East European affairs and in Soviet-American strategic relations and doctrines.

JOHN M. CARFORA

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Globalism versus Realism: International Relations' Third Debate. Edited by Ray Maghroori and Bennett Ramberg. (Boulder, Colo.: Westview Press, 1982. Pp. vii + 237. $25.00, cloth; $12.00, paper.)

Maghroori and Ramberg's collection provides a sophisticated group of essays that deal with international relations theory and with the continuing salient issues of international politics. Much of the controversy in international relations theory during this century has been concerned with two major debates: between the realists and idealists, and between the traditionalists and behavioralists. Maghroori and Ramberg argue that a third debate, between globalism and realism, has emerged and is now the primary concern. The realist model is state-centric and sees the nation-state as the basic political unit in the world. On the other hand, the globalist model sees the world emerging as an interdependent unit with international organizations and multinational corporations, among the actors, replacing the nation-state as the only actor in the world. Optimist-globalists forsee a world state evolving from this system that could handle the growing level of international interdependence, while pessimist-globalists see the negative impacts of interdependence on states.

The preface by James Rosenau accentuates the need to discuss this new "third debate" and ties the 10 essays with the title, "Order and Disorder in the Study of World Politics: Ten Essays in Search of Perspective." These 10 essays indeed are in search of a perspective because they are older theoretical work originally printed as far back as 1969 (with the exception of three essays). They do not provide direct responses to each preceding essay. The essays are grouped into three major clusters: contending globalist (Seyom Brown, Donald Puchala and Stuart Fagan, and Oran Young), realist (Kenneth Waltz, Robert

Gilpin, and Michael Sullivan), and cyclical (Richard Rosecrance and colleagues).

Maghroori's introductory essay describes the major debates in international relations in a clear and concise manner. Seyom Brown's globalist view sees the world as becoming increasingly interdependent, and he forecasts a more ordered global system. In an essay from 1974, Donald Puchala and Stuart Fagan argue that the "national-security paradigm" is a thing of the past, and that integration ("peaceful transnational problem solving") is the basis of current international politics. However, one cannot forget the increase of concern in security politics during the Reagan-Andropov/Chernenko era. Oran Young criticizes Karl Deutsch and notes that interdependence has increased with more international financial institutions, improved telecommunications, and further activities of non-state actors. He argues that an index of interdependence is necessary to explain world politics. Kenneth Waltz contradicts the globalists with the realist view that interdependence exists on a very low level and may not contribute to increasing our chances for peace. Robert Gilpin challenges the globalists and argues that transnational economic actors have not replaced the nation-state as major actors, but have often supported the political views of their home governments. Michael Sullivan recognizes the fact that non-state actors have played an increasingly important role in international politics, but he does not believe that they diminish the importance of the nation-states. Richard Rosecrance and his colleagues take the cyclical view that interdependence has come and gone and come again since 1890, and we are likely to see a continuation of such a cycle.

Maghroori and Ramberg conclude by providing three "'pre-theoretical' subsystemic matrices" to look at the globe in different ways. Matrix number three shows their preferred pre-theory world as more complex than globalists or realists have told us to date and includes an interrelationship between OECD member states, communist countries, developing nations, all of their interrelationships and dependencies and the three models of globalism, realism, and a "tending toward globalism" model. This is basically a pre-theory matrix that can be put into operation in the future if anyone would choose to do so. Perhaps we can look forward to new debates.

MICHAEL A. PREDA

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The Community of States. Edited by James Mayall. (Boston: George Allen & Unwin, 1982. Pp. 189. $28.50.)

How might international reality best be characterized? Although this collection of essays by a dozen British international relations professors is titled *The Community of States*, few of the writers seem to think that *community* is the best designation. Most suggest that at best what we have is a looser *society* of states, some suggest we have really not moved much beyond a mere *system* of states, all agree that states and their governments remain central to our understanding of what is determining the history of our time.

In the Introduction and in a chapter on the international economy, Mayall, senior lecturer in international relations with the London School of Economics and Political Science, argues that "The modern world is thus still a world of states, but states which are forced to coexist in a more uncomfortable intimacy than ever before" (p. 4), and that, alas, "the necessary conditions for the reform of the international community on the basis of distributive justice do not at present exist" (p. 109).

Cornelia Navari develops the view that the world's peoples all seem increasingly to be seeking the same sociopolitical goals of equality (even alikeness), national identity, and modernization. Eventually, this set of universal aspirations may serve as the intellectual and moral basis for a universal community, but to date realizations of these aspirations have been so uneven that the resultant global "community" is still riven with various cleavages, especially along class lines.

Both Christopher Brewin's chapter on sovereignty and Brian Porter's chapter on nationalism remind us of the continued importance of these two related notions for perpetuating states and impeding the emergence of a global community, although both writers acknowledge that complex twentieth-century technological and economic realities are corroding the power—or at least changing the earlier meanings—of both concepts.

Can we look to international law, practical realpolitik considerations, or fear of nuclear catastrophe to push us into more cooperation, more community? Hidemi Suganami is skeptical that international law has moved us, or will soon move us, very far along in the direction of global community. John Vincent feels that we need to perceive practical interests precedent to moving toward community, but he doesn't see us recognizing them sufficiently either now or soon. Barrie Paskins sees some signs of a "terrified community," but thinks most people are confident and comfortable enough with nuclear deterrence to resist the creation of a "genuine international community" based on the *summum malum* (p. 95).

Moorhead Wright hopes that "an ethic of [international] responsibility" (p. 158) might develop, imposing on citizens and leaders moral obligations to promote world peace; he suggests persuasively that this hope is "not unrealistic or utopian," even though such imperatives are obviously not always followed in the contemporary world.

These thoughtful essays and the several others in this slim collection are all gracefully written in the traditional style of British political science. As such, they are informed more by the authors' understanding of European and world history and the works of thinkers like Kant and Marx than they are derived from systematic empirical work or built cumulatively upon the empirical work of others. Thus, some readers will wonder whether the authors are really meeting regularly "to discuss *theoretical* questions in the study of international relations" (emphasis added) as Mayall says in the Preface.

There is no agreement on key terms, either. Most of the authors hold community to be a stage further along in social solidarity than that represented by the term society, however a couple of the essayists use society to denote the more developed form, and one author says he is using the terms interchangeably.

Too, some readers may find it ironical, and even exasperating, that these 12 essayists—all citizens of a country whose sovereign has long been a queen and whose current prime minister is a most forceful woman—all persist in writing sentences such as these on page 18: "Man may be proletarian, or Serbian, or black, but in most current myths he shares one common characteristic. He is found everywhere in a condition of inequality, whereas it is in his basic nature to be equal."

JAMES A. STEGENGA

Purdue University

Neither Cold War nor Detente? Soviet-American Relations in the 1980's. Edited by Richard A. Melanson. (Charlottesville: University Press of Virginia, 1982. Pp. vii + 236. $14.95, cloth; $4.95, paper.)

Very few would question that the future of Soviet-American relations is one of the most important issues in the world today. The nine essays in this volume, with their genesis as background papers to a conference held in 1980 at Kenyon College, attempt to address various aspects of that problem.

The initial essay, "Containment: Its Past and Future," by John Lewis Gaddis is a historical overview of the Soviet-American relationship along with suggestions for conducting it in the future. Gaddis argues that the United States needs to pursue a foreign policy toward the Soviet Union which minimizes vacillations and maximizes consistency. Gaddis argues for a measured and careful look at the vital interests of the United States and a policy of containment which aims at the protection and enhancement of those interests.

"Toward a Method to Madness" by Alexander Yanov and "The Future Soviet Leadership" by Myron Rush analyze the dynamics of internal Soviet politics and leadership. Yanov argues that Russia tends to lapse into increased autocracy in situations of political crisis such as it presently finds itself. To prevent this, the new leadership will have to actively seek to institutionalize the political base of de-Stalinization. Failing this, the Soviet Union will fall into the grip of a new period of autocracy. Rush's essay—which, of course, pre-dates Brezhnev's death—is an attempt to assess the likely Soviet leadership after Brezhnev. The speculation it contains is dated because we now know the outcome of not only the struggle to succeed Brezhnev, but also the struggle to succeed his successor.

In his discussion of "Sino-Soviet Relations in the 1980's" Allen S. Whiting argues that geometry has little in common with international relations. He says that bilateral interaction between any two states has a greater bearing on their relationship than do the ties either has with a third country. His focus, then, is on the Chinese-Soviet bilateral relationship, and he offers some suggestions about how outcomes of that relationship may relate to American foreign policy in Northeast Asia. The following essay, "American Policy in the Strategic Triangle" by Thomas W. Robinson, asks us to view American policy as a three-sided relationship among the United States, the Soviet Union, and China. He argues that the rapid expansion of Soviet power with regard to the rest of the world means that the other major world powers—the United States, China, West Europe, and Japan—need some sort of cooperative arrangement aimed at containing Soviet power.

In "Human Rights and Soviet-American Relations," Kenneth W. Thompson makes the important point that the failure of the Carter administration's human rights policy was due in part to its persistent moralism. He distinguishes between moralism as the tendency to single out one supreme objective in foreign policy and a tendency to apply it indiscriminately and morality that is an effort to map a path through a maze of principles. Thompson does not seek to ignore human rights as a part of foreign policy. He argues for a policy that encompasses morality while recognizing the tensions between the requisites of power and morality. Laurie S. Wiseberg and Harry M. Scoble, in "Human Rights and Soviet-American Relations: The Role of NGO's," analyze political interest groups as they endeavor to raise the human rights issues. They are concerned about effects those efforts have on American policy.

In a truly fascinating essay, "A Neo-Consensus? American Foreign Policy in the 1980's," Richard A. Melanson argues that American foreign policy in the 1980s will be guided by a new foreign policy establishment and will be based on a new consensus. In addition, there may be a new foreign policy debate reminiscent of the one of the late 1940s in the Truman administration. This debate will feature a view of containment based on balance of power considerations against a view of containment based on ideological ones. Melanson clearly prefers the first view over the second. The final chapter is "Power and Prudence in Dealing with the U.S.S.R." by Seyom Brown. It is both an attempt to pull together some of the arguments of the preceding papers and a plea for common sense in our dealings with the Soviet Union.

The topics discussed in this volume are of utmost importance to political scientists. Several of the articles are genuinely insightful. Yet, as a whole, the book suffers faults common to most collections. The essays occasionally stray from the theme and seldom are reactions to ideas raised by others in the collection. The conference for which they were background papers was held in the fall of 1980. This collection was not published until the fall of 1983. The three-year delay in getting the articles into print further diminishes the volume's value. Nonetheless, I would recommend it as a useful addition to undergraduate libraries.

RICHARD HENRY FOSTER

Idaho State University

Cuba in Africa. Edited by Carmelo Mesa-Lago and June S. Belkin. (Pittsburgh: Center for Latin American Studies, University Center for International Studies, University of Pittsburgh, 1982. Pp. ix + 230. $5.95, paper.)

This collection of essays explores three basic questions of Cuban involvement in Africa. The authors attempt to answer why there was an expansion of activity in the 1970s, what the results were for Cuba's international and domestic situation, and what might be the direction of Cuban policy in the near future. The four major essays in the volume were published previously in *Cuban Studies/Estudios Cubanos* (1978, *8*, 10-21; 1980, *10*, (1), 1-36 and 49-79; 1980, *10*, (2), 55-79). They are presented in this collection with two commentaries on each that offer additional perspectives and detail, plus an opening and concluding chapter that present a general framework of analysis and a summary of conclusions. Although some parts of the collection are repetitive in historical detail, the overall result is a thorough and largely balanced examination of questions that have been controversial from the perspective of United States foreign policy under the last three administrations.

Perhaps the most controversial question is the first: Was Cuban involvement in Africa a matter of ideology, Cuban national self-interest, or a result of Soviet pressure? Although Cuba had maintained contacts with a number of African countries from the 1960s on, it was involvement in Angola and Ethiopia in the 1970s that caused growing concern in Western diplomatic circles. In the case of Angola, the evidence seems to show that Cuban involvement in the 1974 to 1976 civil war was initiated from a combination of ideological support for a socialist faction under attack by forces supported by South Africa and other Western sources, and from a desire to further Cuban national interests in third-world leadership and possible long-run economic benefits. The success of this Cuban initiative, however, depended upon Soviet assistance in logistics and materiel, as does the continued Cuban presence in that area. In the later Ethiopian struggle with Somilia over the Ogadan region and with Eritrean insurgency, Cuban assistance appears to have involved prior consultation with the USSR in support of a threatened Marxist regime and in pursuit of Soviet strategic interests in the Horn of Africa. The Ethiopian involvement raised difficult ideological questions, however, because support for liberation and self-determination in Eritrea and the Ogaden were abandoned in favor of realpolitik as cooperation shifted from Somalia to Ethiopia.

If motives for military involvement were not consistent, neither were the results for Cuba in terms of a cost-benefit assessment. Politically there were initial gains in third-world leadership, but at a cost of U.S.-Cuban normalization, increased reliance on Soviet support, and attrition of third-world leadership as support for Soviet adventures in Afghanistan and elsewhere became mandatory. Militarily there were objective victories in both causes plus continued Soviet upgrading of Cuban military hardware and training. The costs are long run as expenditures increase and troops in place become virtual captives of governments with internal problems and their own external agendas. Economically the benefits of increased leverage concerning Soviet aid, trade, and credit conditions have been significant, but at the cost of domestic slowdowns in construction and of trade advantages that could occur with U.S.-Cuban normalization.

The Cuban record in Africa indicates a willingness and ability to pursue revolutionary Marxist goals, but with a degree of realpolitik opportunism that recognizes countervailing restraints as well as the need for Soviet support. This book details these considerations, and with its bibliography and index, is a very helpful source book on related questions.

ROLAND F. MOY

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A U.S. Foreign Policy for Asia. The 1980's and Beyond. Edited by Ramon H. Myers. (Stanford, Calif.: Hoover Institution Press, 1982. Pp. xxv + 144. $10.95, paper.)

From a 1981 conference at the Hoover Institute, these essays by seven regional specialists diagnose the security problems facing the Reagan administration in Asia and prescribe policy changes. Drafted more as policy papers than research studies, they contain sound advice that appears to have been largely ignored.

In his introduction, Myers summarizes many of the specialists' policy recommendations for Asia and promises that they will restore the balance of power in America's favor. In Southwest Asia, Myers advocates strengthening both India and Pakistan against Soviet expansion in a manner likely to reduce their mutual fears. In Southeast Asia, he opposes supporting China so closely that the Vietnamese are driven further into Soviet arms. In East Asia, Myers discourages high-handed pressures on Japan to increase their defense spending. He also urges America to recognize Asia's increasing strategic importance, to involve itself in multiple ties with Pacific-basin states, to elevate India's importance to a level equivalent to China's, and to deploy American naval and air forces in the Indian and Pacific Oceans strongly enough to secure freedom of sea lanes in cooperation with allies.

In chapter 1, Ray S. Cline describes a primary Soviet threat to Asia and discourages closer American security ties to China. Instead, he recommends stronger economic-military relations to the 10 noncommunist states of the emerging Pacific community and especially to such "capitalist-road nations" as Taiwan, South Korea, and Singapore. In chapter 2, Walter R. Andersen examines U.S. policy in South and Southwest Asia and prescribes deployment of naval forces and improved political-military ties to strengthen noncommunist states, especially India. He depicts Pakistan as a dubious choice to anchor a regional defense system and recommends measures to improve relations with a neutral India.

In chapter 3, Leo E. Rose calls for more serious, regular, and high-level consultations with India and opposes dependence on the unpredictable Chinese. To strengthen India, he calls for stationing a new fleet in regional waters and specifies additional policies to improve relations. In chapter 4, Sheldon W. Simon focuses on Southeast Asia and recommends increased naval deployments and decreased diplomatic support of a Chinese United Front policy that drives Vietnam closer to the Soviets. He boldly advocates acceptance of Vietnam's dominant position in Indochina in exchange for a reduction of Soviet influence and lists various policies to secure ASEAN states.

In chapter 5, Donald C. Hellman discusses Japan and Northeast Asia and counsels greater sensitivity to the interaction of military and economic issues in Japan while calling for an imaginative new partnership between the United States and Japan to develop and defend noncommunist neighbors. In chapter 6, Henry S. Albinski considers the role of Australia and New Zealand, urges more appreciation of their security contributions, discourages enlargement of the intimate ANZUS alliance, and encourages an unquestionable commitment of appropriate military resources to the Asia-Pacific basin. In chapter 7, Franklin B. Weinstein reviews United States security policy in East and Southeast Asia and offers a conceptual framework based on a realistic perception of threats and interests. As threats, he perceives both an increased Soviet presence and domestic economic weakness, and he criticizes United States policy for overemphasizing the former and recommends multiple linkages to help with the latter. In Southeast Asia, Weinstein recommends deployment of an appropriate balance of United States and allied forces, acceptance of Vietnamese dominance in Indochina, and normalizing relations with Vietnam. In East Asia, he opposes pressures on Japan to escalate its defense spending for an anti-Soviet force more likely to alarm friends than deter enemies and recommends an expansion of Japanese economic aid programs. In Korea, Weinstein encourages opening diplomatic relations with the North while protecting the South. In China, he warns against providing military aid that serves more as an anti-Soviet gesture than an appropriate security contribution.

The views expressed in these essays remain timely and sensible. Students of Asian foreign policies will gain by reading them.

JOHN JAMES MACDOUGALL

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Soviet Policy toward Turkey, Iran, and Afghanistan: The Dynamics of Influence. By Alvin Z. Rubinstein. (New York: Praeger Publishers, 1982. Pp. 200. $25.95, cloth; $12.95, paper.)

Alvin Rubinstein has written a highly readable history of Soviet relations with Turkey, Iran, and Afghanistan, which will be of great help to those seeking an introduction to the complexities of the USSR's interactions with its neighbors on its southern border. Although the eventual recourse to armed intervention in Afghanistan may stand out as a failure in Soviet diplomatic efforts, most of Moscow's attempts to influence the foreign policies of the three countries, as Rubinstein shows, have paid high dividends historically. Through both subtle inducements and opportune offers of economic assistance, the Soviets have consistently been able to ease military pressures on their borders and minimize regional disputes. In this regard, one particularly fascinating aspect of Rubinstein's account is the way in which the Kremlin has been able consistently to exploit inevitable American mistakes and misjudgments in relations with both Turkey and Iran, with the consequence that Moscow's freedom of movement in this strategic part of the world has been much greater than one might expect. Here, the Soviets can take advantage of the fact that it is always easier to be an opportunistic outsider than a flawless ally.

Although this study is strongest as an historical account, Rubinstein also proposes, in what almost seems an afterthought, to use these case studies as a way of testing various hypotheses about the nature of great power influence. As a rule, however, it is hard to see how the argument is strengthened by the occasional introduction of these artificial constructs. Few readers will be surprised to find, for example, that there is little correlation between the number of missions a small country sends to the USSR and the latter's degree of influence; nor is it all that startling that an increased Soviet presence does not necessarily lead to greater political and military influence. Still, one very interesting point that Rubinstein does raise, and which may perhaps be better developed in another context, is the way in which these smaller states have occasionally been able to exercise their own impact on the policies of the Soviet Union, specifically by threatening to seek the affections of other potential patrons should Moscow not prove sufficiently forthcoming or beneficent. This is an important corrective, because all too often influence is mistakenly construed as merely unidirectional.

Finally, readers may find themselves at odds with Rubinstein's conclusion that the "Soviet Union has achieved its greatest strategic-political triumph in the Third World in the changed policies of Turkey, Iran, and Afghanistan." The evidence he offers would suggest that its impact has indeed been significant. But what about Cuba or Vietnam or even India? This qualification need not detract, however, from the value of a study which focuses on a set of countries on the Soviet periphery whose relations with the USSR (until recently, at least) have been much neglected.

A. JAMES MCADAMS

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South Africa Public Policy Perspectives. Edited by Robert Schrire. (Cape Town: Juta, 1982. PP. viii + 374. R 25.50, paper.)

Many attempts have been made to define public policy, but perhaps one of the most common is that public policy is generally concerned with what governments do, why they do it, and what difference their activities might or might not make to society. In *South Africa Public Policy Perspectives*, Robert Schrire has assembled analyses by 11 authorities on the evolution, content, and impact of some of the important policy outputs of the South African government. The approach of the contributors is very much in keeping with recent trends in policy analysis, to analyze either public policies themselves or governmental outputs rather than the institutions that contributed to their making.

The 11 contributors deal with a variety of South African public policy issues that have both domestic and foreign impacts. They include the administration of justice, political control and personal morality, control of blacks in common areas, homelands, the colored people, the distribution of income, the regulation of labor, industrial decentralization, South Africa's relations with the rest of Africa, the West, and South Africa in the international community.

Even with the slightest knowledge of the South African government budget allocations, one would have expected in such a book analyses of education policy and defense policy. These items account for the first and second largest amounts of expenditure in the budget, and therefore have a vast impact on society. The value of this book would have been increased immeasurably had these two important aspects of South African public policy been analyzed.

One can only single out some of the contributions for discussion. Norman Bromberger writes a pioneering chapter, "Government Policies Affecting the Distribution of Income, 1940-1980." The distribution of income and wealth and governmental expenditure is one of the sources of some of the conflict in Africa, and the issue is likely to become increasingly salient and could well determine the ideological persuasion of a future South African government. Bromberger concludes by saying that for much of the period 1940-1980, "government influenced distribution regressively by protecting whites, and especially Afrikaners, from being competitively 'undercut' by members of the black groups. State expenditures were used to benefit whites and to equip them to meet future black competition" (p. 196). This type of conclusion is likely to have far-reaching implications for the future initiation of redistribution policies.

Another good contribution is that by R.M. Godsell, "The Regulation of Labour." The role of labor, and more particularly black labor, in the South African political economy has gained in importance in recent years as a result of labor's increasing organizational and economic strength. Godsell analyzes the implications of the reports of the Wiehahn and Riekert commissions set up by the South African government to investigate the question of labor and related issues. Here, as in many other contributions, an attempt is made to visualize South Africa's future and the role of labor in that future.

The contribution by Peter Vale, "South Africa and the Changing International Community: Interpreting the Future in the Light of the Past," would probably have been better placed in an international relations text on South Africa and the world. Vale attempts to place the international implications of the South African issue within the broader international political system, and this somewhat removes his contribution from the theme of the book, to analyze South African governmental outputs. The two perspectives are, however, not entirely unrelated.

Very little is known about many areas of South African public policy, and this book is certainly a good attempt to throw further light on these areas. Some of the areas covered, for example, the administration of justice and the development of the homelands, have been covered in other references. However, like so many academic ventures, this book is a beginning in the never-ending research process into the great issues confronting our society.

CLIVE J. NAPIER

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The Dynamics of Interdependence. By Andrew M. Scott. (Chapel Hill: University of North Carolina Press, 1982. Pp. x + 254. $23.00, cloth; $11.50, paper.)

The concept of interdependence has come to embrace virtually the entire scope of global political, social, and economic relationships. One might differentiate varieties of interdependent relationships initially by discriminating between (a) consciously intended and managed interdependencies (such as imperialistic domination, mercantilistic leverage, or foreign assistance tied to greater donor influence and access), and (b) unintended, unanticipated, and frequently unmanageable interdependencies (such as resource depletion, pollution, international monetary volatility, and world inflationary cycles).

It is principally the latter varieties of "apurposive" interdependence that Andrew Scott analyzes in *The Dynamics of Interdependence*. Briefly stated, his thesis contends that international theory lags dangerously behind changing events. The lack of a valid "fit" between theory and reality portends catastrophic collapse into anarchical competition as sovereign states, operating on the outmoded precepts of traditional "preinterdependent" international theory, blindly fail to address the problems threatening the Global Commons. Scott's aim is to provide a measure of collective foresight and to help in fashioning a consensus necessary to move sovereign nations away from preoccupation with outmoded notions of "national interest" and "power politics" toward a more sensitive appreciation of the need for global management of the dynamic processes which are rapidly transforming the international system.

In this literate, artfully crafted text Scott surveys the evolutionary processes of economic and technological change which have knitted together the societies and fortunes of vastly diverse continents. Throughout he stresses the repeated failure of traditional diplomacy to foresee the consequences of institutional changes and political decisions: The Bretton Woods system worsened the North-South gap and exacerbated international debt problems; OPEC price increases had the unintended consequence of fostering world inflation and recession; pursuit of industrialized growth has created unprecedented pollution problems; global population problems are caused by a multitude of microdecisions by individuals. And so on. The world system becomes increasingly vulnerable both because some processes appear beyond the control of private and public actors (variations in ocean currents, disastrous famine due to natural causes, proliferation of carcinogens, ozone depletion) and because some actors deliberately exploit system vulnerabilities (terrorist acts, threats of deliberate debt defaults, trade controls and embargoes). The failure of sovereign states to look beyond particular interests and to transcend their spatial and temporal narrowness bodes ill for any coherent effort at management of these accelerating changes.

This neo-Malthusian scenario of "hyperinteractive apurposive processes" outstripping managerial capabilities, although persuasively argued, is partly counterbalanced by an extended survey of "constraint systems." Such systems represent regimes composed of a variety of actors which regulate interactions and distribute benefits and costs over time. Such systems, ranging from formal organizations (NATO, Warsaw Pact) to more informal arrangements (general agreements to borrow, various aid consortia) bring a measure of order and predictability to world affairs. Many fledgling systems have failed (SEATO, Central American Common Market, East African Economic Community) and others appear moribund (Organization of African Unity, the European Community). Newly emergent systems challenge established regimes (the New International Economic Order versus the Bretton Woods system).

The prescriptive element of this book amounts to a series of propositions urging a substitution of "systemic" perspectives to replace the outmoded notion of "national interest." A sense of obligation must be cultivated as a guide to public policy. Greater measures of global planning and monitoring ("early warning systems") must be implemented. Intergovernmental organizations must proliferate to meet the increasingly complex challenges. The only alternatives to such planning are retreat into isolationist autarchy (impossible for all but a handful of favored countries) or system collapse.

Despite this text's many strengths, there is a disturbing failure to address the obvious possibility that increased planning may in fact exacerbate the problems. The assumption that bureaucratic "constraint systems" must necessarily represent benign, rational intervention flies in the face of a substantial body of theoretical literature (Max Weber, Friedrich von Hayek, P.T. Bauer, and John Maynard Keynes spring to mind) and seems not to fit with ample empirical evidence of inefficiencies in extensively planned economic systems. One may also doubt the viability of such grandiose "reforms" as the New International Economic Order, an idea whose time clearly has *not* come even in the opinion of many third-world elites, much less those of the advanced industrial states. Nevertheless, this is a useful overview of evolutionary trends, a work which synthesizes much of the current literature on international political economy and international regime-formation. It should be readily accessible to both advanced undergraduates and graduate students.

JAMES P. O'LEARY

The Catholic University of America

Japanese and U.S. Policy in Asia. Edited by Gaston J. Sigur and Young C. Kim. (New York: Praeger Publishers, 1982. Pp. viii + 202. $25.95.)

This book consists of papers originally presented in 1980 at a conference on security problems in East Asia. Gaston Sigur is an enthusiastic advocate of the U.S.-Japan security relationship, which, he says, is concerned not only with defense, but also with economic, cultural, political, diplomatic, and educational elements, so that today "no two countries have closer ties in these areas than do the United States and Japan" (p. 5). Both states, Sigur asserts, are global powers and should coordinate their policies in various parts of the world. He deplores the Japanese determination to win economically over U.S. competition at whatever cost, lest the entire fabric of the U.S.-Japan alliance be weakened.

Young C. Kim reports on interviews with 67 Japanese influentials concerning defense issues. The interviews elicited a very wide range of views, but there was considerable skepticism across the board about the reliability of America's commitment to defend Japan. The physical presence of American forces in and about Japan was generally regarded as essential to maintain the credibility of the commitment. Even most of the Japanese who favored increased defense spending did not think that it was necessary except as a way to pacify American demands. The principal reason for opposing more than a slight increase in defense efforts was the fear that Japan might again go down the path of militarism.

W. Scott Thompson is deeply concerned about the Soviet military and naval buildup in the Far East and its implications for the "swing strategy" that contemplates moving the U.S. Navy from one ocean to another as needed. By contrast, James R. Jones, writing on U.S.-Japanese economic relations, seems unbothered by American and Japanese military weakness and urges that the United States follow Japan's example and focus on building up its economy rather than its military establishment.

Mineo Nakajima feels that Japan's 1978 peace treaty with China, which contains an anti-hegemony clause (usually construed as anti-Soviet) provoked the USSR to make an alliance with Vietnam (resulting in Vietnam's invasion of Cambodia and Vietnam-China hostilities) and to fortify the Soviet-occupied northern islands claimed by Japan. The Soviet Union sees a U.S.-Japan-China alliance in the making. Americans are taken in by China's exaggeration of her importance. Japan, Nakajima says, should try to influence America to practice greater restraint in Asia. Tatsumi Okabe warns that China is not suitable as an ally because of its weakness, unpredictability, and its conflicts with other countries. The United States and Japan should strengthen the reliability of their mutual defense treaty in order to prevent the Finlandization of Japan.

Eiichi Imagawa analyzes Japan's economic ties with Southeast Asia and the growing expectations of a larger Japanese political role in that area. Thomas Robinson says that the United States does not perceive the Soviet Union as a major threat in or to Asia and that China is "the major Asian bulwark against Soviet expansionism" (p. 147). Douglas Pike recommends that the United States continue its policy of promoting the cause of regionalism in Southeast Asia. William Barnds warns of the horrendous consequences of another war in Korea and advocates the importance of maintaining the credibility of our commitment to South Korea with the continued presence of American forces if need be.

Although the authors sometimes seem uncertain about the nature of long-term Soviet and Chinese objectives in Asia, the book is strong on the relations of Asian states with one another and with Japan and the United States. Although important shifts of emphasis in policy are suggested by some authors, none advocates the abandonment of the U.S.-Japan mutual security treaty. This book may be read with profit by both specialists and laymen.

THEODORE MCNELLY

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The United States and Pakistan: The Evolution of an Influence Relationship. By Shirin Tahir-Kheli. (New York: Praeger Publishers, 1982. Pp. xxii + 169. $21.95, cloth; $10.95, paper.)

The renewed American strategic interest in Pakistan in the 1980s, following setbacks in Iran and Afghanistan, makes this book a timely addition to the international politics literature.

As a study of relations between the United States and Pakistan during the period from 1947 to 1982, Tahir-Kheli's work is very good. After a generally chronological beginning, chapters 4 and 5 focus more specifically upon two important components of U.S.-Pakistan relations: arms sales and the issue of nuclear proliferation. Throughout, the author provides stimulating and insightful commentary, frequently taking issue with other scholarly accounts and asserting her own interpretations. She goes further than most other writers, for example, in taking seriously the accusation that the United States was involved in the 1977 coup that toppled Bhutto (p. 72). She also tends to place greater emphasis than most upon external factors to explain such domestic developments as Bhutto's purge of the PPP leftists in the early 1970s.

Tahir-Kheli supports her arguments with numerous quotations from interviews with high-ranking officials in the United States, Pakistan, and India. For the most part the book is free of factual or terminological error, the most notable exception being a reference to Air Marshal Asghar Khan's Tehrik-i-Istiqlal as the "Isteqlal-e-Pakistan" party (p. 70).

This book is part of a series of studies of influence in international relations edited by Alvin Rubinstein and might profitably be read as a companion piece to another recent work in the series, Robert C. Horn's *Soviet-Indian Relations: Issues and Influence* (Praeger Publishers, 1982). Accordingly, Tahir-Kheli utilizes her historical and case study materials ostensibly to test, but really more to illustrate, a set of seven hypotheses concerning influence between the two countries under consideration.

In this respect, the book engenders a number of critical questions. First, it is never entirely clear whether the subject is American influence over Pakistan, Pakistani influence over the United States, or both. The initial formulation (p. xi) implies an emphasis upon Pakistani influence: "how does a small weak state, with regional concerns, in an alliance with a large, powerful state having international goals, operate with any range of freedom, and indeed, attempt to maximize its influence?" On the other hand, all of the hypotheses are framed in terms of American influence over Pakistan. Discussion elsewhere treats both sides of the relationship.

Second, influence is narrowly defined (following Rubinstein) as A affecting the behavior of B "to the policy advantage of A" (p. xii). Clearly, American arms aid to India in 1962 influenced Pakistan's decision to establish relations with China (pp. 10-11), but this form of "negative influence" is not open to consideration as such within the operating definition.

Third, even granting the definition, how does one measure influence? Is a country influential only if it fully accomplishes its policy goals, or can one discern degrees of influence? Tahir-Kheli rejects the hypothesis that "the greater B's economic dependence on A, the greater A's influence on B" by explaining that Washington failed to get Pakistan to fall in line "with the two main aims of its policy, namely, regional defense, and containment of the Soviet Union" (p. 155). However, she argues shortly thereafter that the United States "can be said to have had a modicum of influence over Pakistan" during the same period because Pakistan was persuaded not to seek aid elsewhere and Washington was able to "line up Pakistani policy on its side on international issues" (p. 157). How then can Pakistan's economic dependence be regarded as unrelated to American influence?

Despite these questions and criticisms, *The United States and Pakistan* is a valuable addition to the literature on South Asian international politics, American foreign policy, and the concept of influence in international affairs.

WILLIAM L. RICHTER

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The Politics of Indecision: Origins and Implications of American Involvement with the Palestine Problem. By Robert Daniel Tschirgi. (New York: Praeger Publishers, 1983. Pp. xvii + 341. $29.95.)

This book seeks to assess the responsibility of the Roosevelt and Truman administrations for the bloody breakdown of order in Palestine in 1948 through a study of the evolution of United States policy during the decade between the issuance of the 1939 White Paper and the United States' recognition of Israel. The book presents a reasoned, judicious account, one that is rich in facts, sharply focused, and based on the skillful use of many primary sources. It is noticeably devoid of the demonology and conspiracy theories that mar many studies of this and related topics.

Tschirgi divides his study into three parts. In Part 1, he uses the issuance of the 1939 White Paper to present succinctly the prior history of the Palestine problem, the new campaign by the Zionists to mobilize Jews and non-Jews in America, and the perspectives on Palestine of the American public and key government institutions—the presidency, Congress, and the State Department. Parts 2 and 3 comprise a narrative history and are organized chronologically. Part 2 treats with the attempt by the Roosevelt administration to deal with the Palestine problem between 1939 and 1945 in the context of World War II and growing Zionist activity in America. Part 3, the longest part, is concerned with the Truman administration's attempt to deal with the issue from the end of the war until the recognition of Israel in May, 1948, an attempt shaped by the deteriorating political-military situation in Palestine, the emergence of the United States as a Great Power with interests in the Arab World, the beginnings of the cold war, and the increasing efforts of Zionism to use domestic political pressure to commit the administration to a pro-Zionist policy.

It is Tschirgi's thesis that American policy during this decade precluded an alternative to a solution of the Palestine problem by brute force in 1948, and that in pursuing this policy the Roosevelt and Truman administrations acted callously and irresponsibly. The policy was one of "calculated indecision," he says. "American 'indecision' entailed closely planned and tightly executed tactics designed to evade the core of the Palestine question: whether Arab or Jew . . . would dominate politically in that country" (p. xvi). Tschirgi marshalls a mass of evidence to support convincingly his argument that the American government first tried to put the Palestine issue in abeyance and then sought repeatedly to avoid taking decisions at junctures that required decisions. More important, the government refused to commit itself to a long-range policy based at least in part on the unfolding situation in Palestine; instead, it reacted in an often contradictory fashion to pressures and events of the moment, and short-term domestic political considerations came to prevail increasingly.

Tschirgi correctly puts much of the responsibility for this failure in policymaking on the shoulders of Presidents Roosevelt and Truman. He is more critical of Truman than Roosevelt, and claims that, unlike Truman, Roosevelt at times "viewed the Palestine problem through the dispassionate eyes of a political realist and the imaginative perspective of a visionary" (p. 257). Unfortunately, Tschirgi does little to document this difference between the two presidents. As for his "imagination" and "vision," what is one to make of Roosevelt's statement that he considered eventually "settling the Palestine question by letting Jews in to the limit that country will support them—with a barbed wire around the Holy Land" (p. 75)? No, both presidents failed in Palestine, and just about equally. If Truman came off looking worse than Roosevelt, it is largely because he was there when the string ran out on Palestine.

ROBERT BURROWES

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Militarization and Arms Production. Edited by Helena Tuomi and Raimo Vayrynen. (New York: St. Martin's Press, 1983. Pp. vii + 310. $32.50.)

The Tampere Peace Research Institute (TAPRI) participated in a United Nations research program conducted from 1978 to 1981 on the relation between disarmament and economic development. In the course of their work on the United Nations study, two researchers associated with TAPRI, Helena Tuomi and Raimo Vayrynen, felt that better international contacts should be established among scholars engaged in research on economic development and disarmament. The TAPRI international seminar born of that concern, "Arms Production and Development," was held in 1981 in Tampere, Finland. *Militarization and Arms Production* contains a number of contributions to that seminar along with some essays the editors "regarded as useful."

As the editors write in the Introduction, the expressed purpose of this volume is "to sharpen our analytical tools and deepen our understanding of the disarmament/development relationship in various types of countries" and encourage "us in our search for a more secure, socially just and less militarized world." With such an ambitious and laudable goal, the book's failures are especially unfortunate.

Militarization and Arms Production contains 14 chapters organized into 5 sections and 8 appendixes. There are several interesting chapters. Milton Leitenberg submits an essay published in *Public Policy* (1981, *29*, 437-471) on U.S. and USSR arms control policies. His discussion of reductions in U.S. and USSR military expenditures from 1963 to 1965 is valuable. Michael Klare presents a short but clear analysis of the Reagan administration policy on conventional arms sales. Signe Landgren-Bäckström discusses transfer of military technology to lesser developed countries. Finally, Jan Oberg thoughtfully criticizes the TAPRI seminar leading to this volume for being "less than successful" (p. 298) in taking up questions central to peace research. Oberg asks that peace researchers keep in mind the relevance of peace research to peace, the audience for peace research, and the benefits others can gain by listening to peace researchers.

The two main problems with this book are the lack both of an integrating essay and a coherent focus for the individual authors. The essays cover diverse topics and are generally not relevant to the editors' stated purpose. Some essays describe particular policies, such as Clovis Brigagao's study of Brazil's nuclear energy policy. Other essays present utopian and naive proposals, such as Michael Brzoska's notion of a "joint conversion policy" which would entail the Federal Republic of Germany contributing to a fund to assist other West European nations in conversion from military to nonmilitary manufacture.

The audience for *Militarization and Arms Production* would seem to be primarily advocates of reduced military expenditure in the West; yet the arguments and essays presented are too weak to be of much political use. There is also a lack of balance in the discussion, as little is said to elucidate the problems posed by Soviet bloc military expenditures. The potential reader would do far better to consider separately other published work of Leitenberg, Klare, Landgren-Backstrom, and Oberg.

MARK D. MANDELES

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Northeast Asian Security after Vietnam. Edited by Martin E. Weinstein. (Urbana: University of Illinois Press, 1982. Pp. xii + 182. $17.50.)

The essays in this volume were originally prepared for a conference on Northeast Asian Security held in the spring of 1978 at Harvard University. Enough time had passed since the collapse of the Saigon government to begin to question why the rest of the American dominos in Asia, wobbling perhaps at times, had not yet started to fall. This collection of essays by specialists in Asian security affairs suggests that the answer is that Northeast Asia comprises a distinct regional subsystem, displaying certain self-regulating features and, despite frequent shocks, so far able to maintain a precarious equilibrium.

The first section of the book provides an overview of this Northeast Asian system. Focusing on the interplay of domestic and international factors, Akira Iriye traces its development from the "Yalta system," through the increasingly tense face-off between the United States and China after Korea, to the present "three-tiered system" in which the United States serves as a strategic counterweight to the Soviet Union, Japan has inherited the mantle of "capitalist internationalism," and China plays the role of stabilizer and spokesman for the Third World. Masataka Kosaka also emphasizes domestic politics, observing that the collapse of Saigon reverberated mainly in psychological estrangement from Asia among the American people. He perceptively analyzes "de-Americanization" in Japan in the early 1970s and discusses some neglected aspects of the stabilizing role of American forces in Korea.

The second half of the book deals with national policies and perspectives. Makoto Momoi paints an alarming picture of the political impact on Japan of the Soviets' military buildup in the Northwest Pacific. Although unabashedly geopolitical, his discussion of Soviet options takes on a chilling immediacy in the wake of the downing of KAL flight 007. Donald Zagoria, looking at Moscow's "Eastern problem," reaches almost exactly the opposite conclusion. He feels that the Soviet Union has failed in Asia because of its conflict with China, its reliance on military power, its ham-fisted inability to exploit political opportunities, and the relative stability of the area. Even so, Zagoria cautions that many contingencies might arise that could destabilize the region and provide Moscow with new opportunities.

Chae-Jin Lee reviews the shift from hostility to accommodation in Chinese-Japanese relations. Lee sees this as exerting an overall stabilizing effect and hopes that cooperation may eventually be extended to reducing tensions on the Korean peninsula. Daniel Okimoto discusses Japan's nonnuclear policy, controversies surrounding American weapons, and the circumstances in which Tokyo might decide to develop its own bomb. Okimoto argues convincingly that a nonthreatening Japan makes a major contribution to regional security, but avoids discussing burden sharing and seems to muddy the distinction between a rearmed and a nuclear Japan. In the final essay, Bae-Ho Hahn surveys sources of tension in the American-South Korean relationship and the unsettling effects of the Carter administration's troop withdrawal plan. Caught in a vicious circle of insecurity, repression, and isolation, President Park eventually succumbed to an economic downturn and to the social barriers of extreme authoritarianism mentioned, by way of exculpation, by Bae-Ho Hahn.

Although there is some overlap in content, the essays generally complement each other and together provide broad coverage of the political scene in Northeast Asia after Vietnam. Martin Weinstein's thoughtful introduction calls Americans to task for our ignorance of Asia and highlights the main themes in the book. Only two essays appear to have been updated, but most of the others are historical pieces that remain useful. In sum, this collection will be a valuable reference for anyone interested in security policy and international politics in East Asia.

JOHN MERRILL

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Towards the End of Isolationism: China's Foreign Policy after Mao. By Michael Yahuda. (New York: St. Martin's Press, 1983. Pp. xiv + 279. $25.00.)

Despite the burgeoning scholarly output in recent years, the study of Chinese foreign policy still lacks coherence; it advances and contends in divergent directions in an atheoretical, noncomparative, and noncumulative manner. The problem is evident in the conflicting assumptions and conclusions about the wellsprings of the constancy as well as the change in Chinese foreign policy. Yahuda, an English writer, presents his work as the first book to examine in depth the continuities and changes in post-Mao Chinese foreign policy. For this task he employs a framework that distinguishes between the "strategic geopolitical" and "societal" dimensions of Chinese interactions with the outside world. The book concludes with a questionable assertion that the most striking aspect of China's foreign policy is its continuity with the Maoist era, particularly in the strategic realm, but that new elements of change are evident in China's "diplomatic style."

Ironically, the strength of the book lies in the background analysis of the roots of the post-Mao foreign policy in Part 1. Yahuda makes a persuasive case for the "Mao in command" thesis. His analysis of Mao's geopolitical thinking is remarkably immune from the "love-hate" swings of emotion, perception, and even fashion especially pervasive in American China scholarship. In this book, China or Mao is rarely used as a whipping boy for the outside observer's earlier misconceptions. Instead, Yahuda judiciously captures the dialectics of the theory and practice of Mao's geopolitics. Although the first half of the book provides an excellent summary for the uninitiated, the specialist in the field may come away with a sense of *déjà vu,* a sense of having already traversed the familiar terrain well charted by John Gittings, Franz Schurmann, Stuart Schram, and Allen S. Whiting.

The performance of the book as a whole, however, falls considerably short of the promise. The book breaks no new ground in terms of methodological sophistication, theoretical advancement, thorough empirical research, new sources, or interpretation. Instead, the book is an interpretative history of Chinese foreign policy from 1949 to about the end of 1982 with emphasis on strategic and economic policies—almost a revised and updated edition of Yahuda's earlier work, *China's Role in World Affairs* (St. Martin's Press, 1978). Yahuda warns at the outset that "changes in Chinese policy arise from internal developments that continue to be unknown and perhaps unknowlable" (p. ix), yet he often ignores his own caveat and succumbs to *ex cathedra* pronouncements that begin: "There can be no doubt . . .," "there can be little doubt . . .," or "it is patently clear. . . ."

The title (and organization) of the book is misleading. The end of isolationism came in 1970 and 1971, not a new movement initiated during the post-Mao era. What Yahuda really means is not "isolationism" but "autonomy" or "independence." Substantively, more than half of the book is devoted to Mao's legacy. The analysis of the post-Mao Chinese foreign policy is inadequate and discursive, lacking both a coherent structure and a sustained argument. Yahuda's argument is often difficult to follow. Still, his main analysis has not fully captured all the turns and twists, especially the shift since mid-1981 from a policy of strategic collaboration with the United States—one that verged on a *de facto* alliance—to a more detached and independent foreign policy. It would have been more tenable to turn his conclusion around by suggesting that the changes are evident in post-Mao geopolitical orientation and behavior, whereas China's "diplomatic style" has remained relatively constant.

Making no use of the vast literature on comparative foreign policy and international relations and lacking sure footing in comparative analysis, Yahuda picks and drops various concepts arbitrarily. Based on a Stalinist notion of power—how many divisions does the Pope (and Mao) have to project military forces beyond borders?—Yahuda is puzzled as to why China has become such an important factor in Soviet and American strategic calculus. Surely, one cannot ignore the fact that China constitutes an intermediate zone of sorts in the Eurasian continent in the superpower rivalry. Yahuda is also insensitive to the cognitive principle that perception defines reality in global politics and "power" as the ability to affect the behavior of the target party often lies in the eye of the beholder.

Yahuda often commits the fallacy of literalism by accepting, and often amplifying, China's policy pronouncement as policy performance. There is no sustained attempt to examine the extent of word/deed disjuncture by drawing a conceptual distinction between ideals, principles, and orientations expressed in the policy pronouncement phase and actual behavior made manifest in the policy implementation phase. Given the projected self-image of Chinese foreign policy as one of unvarying constancy and continuity, the case for such a conceptual distinction and a behavior-based empirical analysis should not be dismissed as spurious scientism. Consider, for example, Yahuda's uncritical references to China as a third-world nation merely accepting China's own rhetorical self-identification and giving only a parenthetical treatment of China's third-world policy. In a behavioral sense, China acts as a "group of one," not as a team player of such third-world caucuses as the Group of 77 and the Non-Aligned Movement.

Unfortunately, this book shows the common conceptual and methodological weaknesses of the traditional, Sinocentric study of Chinese foreign policy without its major compensating strength—the use of Chinese-language sources. Yahuda relies exclusively on what is available in English. A close perusal of the more recent scholarly articles in the newly inaugurated Chinese international relations journals and yearbooks coupled with a greater use of behavioral data would have provided a more reliable basis for delineating the logic of post-Mao Chinese foreign policy.

SAMUEL S. KIM

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Normative Theory

The Tactical Uses of Passion: An Essay on Power, Reason, and Reality. By F.G. Bailey. (Ithaca, N.Y.: Cornell University Press, 1983. Pp. 275. $29.50, cloth; $9.95, paper.)

The title of this work is a provocative misnomer for what is an important book. F.G. Bailey is concerned with reason rather more than passion, but his real interest is with power. He analyzes how it is that some people get their way with others and other people do not; his special interest is in the committee world that is the characteristic mode of bureaucratic decision making. A political anthropologist and the author of *Strategems and Spoils* (Schocken, 1969) among other works, Bailey bases his analysis on an unusual combination of sources: Indian parliamentary debates, a California town council meeting, novels by C.P. Snow and John le Carre, and his own observations of university proceedings. In each of these settings, Bailey examines the uses of different rhetorical devices to determine what rules apply for winning arguments by using passionate appeals.

Passion is the "dirty secret" of administrative argumentation, because in the Weberian orthodoxy, reason is supposed to control in making decisions. Displays of emotion, however, play a much larger part in governing formal organizations than we are ordinarily prepared to admit. As a persuasive device, passion seeks to circumvent logical thinking when such a process will not yield the desired outcome. Tactically, passion is powerful because of its authenticity. Revealing one's inner self to another can provide a measure of affective control over another's evaluation of events. "The passions, in these circumstances, are a route to power" (p. 24). On the other hand, mismanaged passions can excite distrust and derision. Bailey's goal is to spell out rules "that advise on the tactical use of displays of passion by oneself and of the provocations of such displays in other people" (p. 25).

Bailey brings a perceptive eye to the intricacies of committee life as he distinguishes the behavior associated with its different types: ad hoc, elite, and arena. The last committee is populated with representatives from different administrative constituencies and is the locus of often sharp power struggles for limited resources or for formulating institutional policies. The conduct of such a committee is regulated by a "sophisticated code"—understood and manipulated by its leaders, recognized and learned by those who would lead—which dictates appropriate behavior and strategems. Although recruitment procedures usually suffice to produce members who abide by the code, the committee's leadership will demand compliance from an unknowing or recalcitrant recruit. For example, the simple code of a "rhetoric of assertion" must give way to the more complex style of practical reasoning and bargaining.

The existence of a sophisticated code indicates that agreement exists about fundamental values, and the points of dispute are over the best means to solve a particular problem. Such broad agreement is a necessary and expected characteristic of a decision-making group that must choose among competing organizational factions while keeping conflict tightly within bounds. On the other hand, a high degree of sophistication can also mask a simple unwillingness to entertain certain ideas or to include certain people; the code defines *a priori* certain kinds of appeals and appellants as "unreasonable" or "unacceptable." Bailey sees this pursuit of power, especially in the rationalistic culture of modern bureaucracies, as an elaborate game in which certain devices for aggrandizement are sanctioned and others are not. His description of the sparring between George Smiley and his administrative rivals within the British intelligence community of *The Honourable Schoolboy* shows Bailey the political anthropologist dissecting the norms of our political culture and proposing hypotheses that seek to explain their meaning and use.

The Tactical Uses of Passion is to be highly recommended to all political scientists for its theorizing about small-group power and organizational rationality. Students of public administration will be most grateful, however, for the invaluable insights Bailey offers about the psychology of the higher bureaucratic culture. The book should be widely read, if for no other reason than the understanding that it can provide of the maneuverings typical of academic departmental meetings. Because attendance at these is a largely inescapable fate, *The Tactical Uses of Passion* is strongly recommended as a helpful anodyne.

KEVIN V. MULCAHY

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Moral Responsibility in Conflicts: Essays on Nonviolence, War, and Conscience. By James F. Childress. (Baton Rouge: Louisiana State University Press, 1982. Pp. xiv + 224. $20.00.)

In these essays Childress examines how a person as a moral agent justifies his response to an unjust or evil policy when his response is nonresistance, nonviolent resistance, violent resistance within limits, or violent resistance without limits. Childress's primary concern is to inquire into what is involved in moral justification by focusing on the nature, process, and context of justification in situations of social conflict. He begins by asking what kind of justification is offered by such people as the early Quakers, Gandhi, and King who chose to use nonviolent resistance in protesting an injustice. It is trust, Childress finds, that operates as the central idea in their arguments for nonviolent resistance. The nonviolent resister "expresses" trust towards the opponent by voluntarily subjecting himself to physical risks that the opponent will "respect moral limits" in responding to the resister's demand. He also seeks to evoke trust from the opponent that the latter can rely on the former to act in a civilized way in their confrontation. Even when the opponent is untrustworthy, the resister justifies his expression of trust by appealing to "therapeutic" or "redemptive" trust in that this expression of trust will encourage the trustee to be trustworthy and will have a desirable moral impact on him in the future.

Childress then analyzes the arguments against nonresistance and nonviolent resistance by subjecting Reinhold Niebuhr's realistic critique of pacifism to critical scrutiny. He does an effective job of pointing out numerous inconsistencies, ambiguities, and difficulties in Niebuhr's position against pacifism. According to Childress, Niebuhr attacked primarily two types of pacifism: "absolute religious pacifism," which requires nonresistance, and "pragmatic pacifism," which supports nonviolent resistance. Niebuhr opposed absolute pacifism because it is dictated by the Christian ideal of love, an ideal that is unachievable in history and hence one that cannot be used in dealing with questions of social policy as long as man remains a sinner. As a result, nonresistance becomes an act of irresponsibility in ignoring the political world of justice on which we all depend. Niebuhr criticized pragmatic pacifism particularly for its pragmatic claim that nonviolent resistance is effective, a claim unsupported by the facts of human history. Against Niebuhr's view that rejects any "intrinsic" difference between violence and nonviolence, Childress presents a persuasive case that there is an intrinsic difference between them; moreover, he discovers that Niebuhr did not, and could not, consistently deny their intrinsic difference.

Taking up the question of how one justifies violent resistance within limits and violent resistance without limits, Childress explains traditional just-war criteria and their grounds and defends these criteria by reference to the "prima facie duty of maleficence," the duty not to injure or kill others. After a careful analysis of the notion and implications of *prima facie* duties, he advances an important thesis that both *jus ad bellum* (the right to go to war) and *jus in bello* (right conduct in war) presuppose—and hence are subject to significant constraints imposed by—the *prima facie* duty of maleficence. As to what specific restrictions should govern the conduct of war, Childress turns to Francis Lieber's works to discuss how various rules of *jus in bello* can be explained and justified, demonstrating fine historical scholarship in reconstructing Lieber's arguments. Addressing the issue of conscientious objection, Childress investigates the nature of conscience by asking what it means to appeal to conscience when conscientious objectors refuse to participate in war. The key feature in the appeal to conscience, he maintains, is a "personal sanction," a sanction involving the pain of the "loss of selfhood, integrity, and wholeness" visiting on the self from disobeying one's conscience. He holds that because it must protect conscientious objectors from such a personal sanction, the state cannot justify denying conscientious objection unless it can show compelling interests. In his last essay Childress discusses policies towards conscientious objectors. He is here mostly interested in refuting the arguments often heard in opposition to the exemption of selective conscientious objectors. He contends that there are no relevant differences between universal conscientious objectors and selective conscientious objectors to warrant their different treatment.

Childress, drawing extensively and skillfully on important concepts, principles, and distinctions found in the writings of contemporary philosophers and theologians, does a superb job of identifying and clarifying significant issues and implications in our debates on the question of violence and provides a useful, fertile framework for reflection and research. This is a work displaying considerable analytical power and deep moral insight, a book that can be profitably read by students of contemporary political philosophy.

HOWARD H. PARK

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John Locke's Moral Philosophy. By John Colman. (Edinburgh: Edinburgh University Press, 1983. Pp. viii + 280. $27.50.)

By "moral philosophy" Colman means a theoretical account of moral action that shows why such action is reasonable. He distinguishes Locke's moral philosophy from the "secular voluntarism" of Hobbes and the "theological voluntarism" of certain protestant thinkers. Its "leading idea" is "that there is a law embodying the moral duties of mankind which, although it ultimately derives from God, is accessible to human reason without the aid of revelation" (p. 9). It is Locke's constant assertion "that the revealed moral law and the law of nature are identical in their content" (p. 39). In the decisive sense, Colman understands Locke to carry on the tradition of Christian rationalism represented by Thomas Aquinas and Richard Hooker.

According to Colman, however, Locke does break with this tradition in some respects. His theory lacks the "metaphysical world picture" characteristic of medieval rationalism, and he denies that there are any "essences" that exist within the compass of the understanding of human beings except those which they make themselves. This rejection of essentialism generates an ethical relativism that denies that the ends of human striving can be ranked according to some metaphysical standard.

As a result, Locke, unlike Aquinas or Hooker, views rational action as nothing more than rational hedonism. Colman criticizes Locke's theory for being unable to account for the motivation of those who choose virtuous activity for its own sake (p. 228). Even Locke's understanding of human pleasure is too "sensationalistic" to do justice to the phenomenon (p. 232).

In some places, Colman seems to think that rational hedonism is a sufficient motivation for adhering to the law of nature. He explains that "the rational individual will acknowledge that, even though the observance of it is at times to his disadvantage," his "successful pursuit of personal happiness" requires "the social stability and security" which general obedience to its rational precepts provides (pp. 235-236). This explanation does not make it clear why the rational individual might not make himself an exception to the general rule in an appropriately clever way.

Consequently, Colman is ultimately compelled to conclude that, for Locke, "were there no chance of divine reward or punishment, it *would* be unreasonableness to act virtuously when such action contravened our private happiness" (p. 226). The possibility of divine reward or punishment is the only sensible reason for obeying the moral or natural law because, from the perspective of a rational hedonist, this-worldly or merely natural sanctions against its disobedience are insufficient. Yet, for Locke, the very definition of law includes the existence of effective sanctions.

In order to defend the position that the law of nature is truly a law, Locke must show that at least the possibility of divine reward and punishment can be known by unaided human reason. According to Colman's account, Locke finds such knowledge through reflection upon the ultimately unsatisfactory character of every form of merely human happiness. Human experience points to a form of happiness that totally transcends that experience and hence does not share its necessary imperfections (pp. 226-227). One wonders whether Locke really expected the genuinely rational hedonist to be persuaded by this argument. The fact that a desire, however fundamental, is not satisfied in this life does not mean it is satisfied somewhere else. The fact that human beings desire immortality does not mean they will really achieve it.

One becomes perplexed when Colman reveals that Locke also believed in "a richer morality," one based on the New Testament Golden Rule, but "he could not see how that rule was to be established as the fundamental law of nature." He could not make Christian and other "precepts enjoining the doing of positive good to others" compatible with a morality which is grounded in rational hedonism (p. 203). How can this incompatibility be reconciled with Locke's constant assertion that there is no conflict between the law of nature and revelation when both are properly understood?

Because of his perception of the incompleteness of Locke's moral theory, Colman does not think him to be a first-rate moral philosopher (p. 8). If, as I think but contrary to his own view, Colman's evidence points to Locke's radical inconsistency, is he, at best, a third-rate moralist? Another possibility, which Colman dismisses without argument, is Leo Strauss's interpretation of Locke's inconsistencies as too obvious to have been unconscious. Locke must have both an exoteric teaching, which appeals to the traditional prejudices of the Christian-natural law tradition, and an esoteric one, which reveals Locke's fundamental acceptance of modern, Cartesian-Hobbesian atheism. To me, Colman's position that "a non-theistic ethic is foreign to Locke's thinking" (p. 38) is untenable. Still, he is right to believe that Locke's moral intention cannot be divorced from Christian presuppositions.

I do not want these critical remarks to detract from my conclusion that Colman's book is an admirable attempt to understand the whole of Locke's work from comprehensive moral perspective. It deserves in some ways to be ranked with the best of Locke scholarship in recent years.

PETER AUGUSTINE LAWLER

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Politics and Human Nature. Edited by Ian Forbes and Steven Smith. (New York: St. Martin's Press, 1983. Pp. x + 198. $25.00.)

The relationship between political philosophy and psychological theory is a traditional concern and traditional problem for political theorists. Attempts to engage in sophisticated discussions of human nature can reveal the political theorists' (almost necessarily) amateur status or can result in a fascination that leads farther and farther from political and social issues. Even if the political theorist returns from long journeys into psychological theory, it is not clear what such labors have gained. To base a political theory on a detailed knowledge of Freudian, Jungian, Reichian, Skinnerian, Laingian, or sociobiological theory simply seems to involve oneself in hopelessly controversial assumptions.

It is no wonder that some of the most important political philosophers of the last few centuries have tried to do without a theory of human nature. An analysis of human nature is irrelevant to most immediate political problems and may seem to make others needlessly complicated and controversial. Moreover, it is inconsistent with the pretensions of a political science to engage in the endless arguments over the selfish or sociable nature of people. Both politics and science were behind Hume's attempt in the *Inquiry Concerning Morals* to avoid arguments about human nature. Rawls followed Hume's lead and denied that his theory depends on assumptions about human nature. Recent interest among political scientists in using economic models of rationality has reinforced the behavioral bias against these questions. At least as strong as antipathy to discussions of human nature has come from Marxism, which sees behind any belief in a human nature another ideological ploy. Sociobiology's influence and apparently conservative political conclusions have only exacerbated this antagonism.

Politics and Human Nature grew out of a conference panel organized by Graeme Duncan to reassert the importance of human nature for the study of politics. The essays that make up this volume have the problems and benefits associated with panel papers. Although they are rarely as fully developed and meticulously argued as articles would be, they are short, informative, and sometimes lively. It is useful that they have been published together, for their individual weaknesses are more than compensated for by their sustained consideration of a single topic.

The argument common to them all is that "the concept of human nature underpins political analysis" (p. 1). These essays, then, are less concerned with putting forward their own views of human nature and the consequences for politics than with discovering or understanding the psychological assumptions made by others. The essay by Paul Smart on the theory of human nature found in J.S. Mill's *Logic*, and the essay by Jeffrey Sedgwick and George Sulzner on Woodrow Wilson's view of human nature and its importance for understanding civil service reform in America are particularly interesting and informative. Among other topics covered are psychoanalysis, feminism, theories of work, and theories of war.

Uncovering the controversial assumptions about human nature that are behind a political philosophy is a favorite left-wing enterprise, and this perspective seems to inform most of these essays. However it is to the authors' credit that the essays on this subject turn their analyses on themselves and consider what a Marxist or radical theory of human nature would be rather than escaping into a vague idea of human malleability. Thus, Graeme Duncan writes that "the radical, who focuses upon potentialities or capacities, need not claim that the potentialities of everybody are the same and in principle unlimited, that anybody could be the same as anybody else, that everybody could realize all their potentialities, or that anybody could be anything. Radicalism need not accept a choice between environment and genetic inheritance, and opt for the blank sheet" (p. 10). This theme is also present in provocative essays by Ian Forbes and Vincent Geoghegan. In short, for political theorists who are unpersuaded that questions of human nature are irrelevant or unscientific, and particularly for those concerned with theories of human nature consistent with political and social change, this volume is of considerable interest.

Thomas A. Horne

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The Modern Liberal Theory of Man. By Gerald F. Gaus. (New York: St. Martin's Press, 1983. Pp. viii + 312. $27.50.)

Gerald Gaus argues that there is a modern liberal theory as distinct from an older, less rich version, and that modern liberalism is best understood from the standpoint of its view of human nature. Gaus accepts C.B. MacPherson's dividing line between an older liberalism (stemming from Hobbes to Bentham), which places economic man at the core of its conception of persons, and the modern liberalism which has a more expansive view of persons and which Gaus locates in the writings of J.S. Mill, T.H. Green, L.T. Hobhouse, Bernard Bosanquet, John Dewey, and John Rawls.

Using these six thinkers, Gaus provides a comparative analysis that demonstrates how the "specifics" of the modern liberalism's view of human nature is the basis of a "justification for those institutions characteristic of liberal democracy" (p. 7). The book is an important contribution simply for the fact that it nicely reviews the writing of the "older" modern liberals, who have all but been forgotten in the recent fascination with Rawls and Nozick.

For the modern liberal, persons are understood as unique beings who, in seeking to realize the fullness of their selves, place a priority on the "higher" as opposed to the "lower" capacities. A rich self highlights not his or her quest for bodily pleasure, but the hope of realizing a person's "complex intellectual and artistic potential" (p. 106). Further, persons seek a more complex self in the context of a personality that is an integrated ensemble of all of its aspects. A personality out of balance is one that seeks to develop only a few of its capacities and thus denies the growth of all the others.

But the hope for an integrated, rich, and complex self is hindered when, in actual life settings, persons are forced to become specialists, thus emphasizing one set of capacities and allowing others to languish. This experience leaves persons with a sense of incompleteness and a yearning for a fuller expression of their self. Persons may compensate by finding social relationships that provide to the participants an opportunity to "participate in lines of development which we either had to forego (in order to specialize) or of which we were not capable" (p. 106). Modern liberal persons are well suited for social life not only because their rational self interest requires them to establish relationships of mutual interdependence, but also because they need to experience the affective ties and common bonds that social life provides in its best (communal) form. In this setting, persons experience "fraternity" with others, and they acquire a more balanced self.

The problems for modern liberalism begin here. The modern liberal conception of persons that goes a long way to embracing community, still espouses a view of persons which finds community interests often in conflict with and in opposition to the needs of the fully developing self. The modern liberal hopes to balance both individual and community needs by ensuring basic rights, opportunities for political participation, public education, economic fairness, and decency in the industrial workplace.

Gaus's account is solid and convincing. My complaint is that Gaus seems to slide over the fact that modern liberalism has inherited some of the problems of the older variety; in particular, a too-concentrated focus on rights to the exclusion of a respect for duties. In a rights-based society, persons, even deeper selves, may create a fragmented social environment as they pursue the respective good or life plan to which they may claim a right.

Gaus discusses Rawls's view of society that requires that persons have a commitment to just principles. Indeed, Rawls argues for a duty to justice that supersedes in importance other interests and sentiments. Without it, society may become fragmented and riven with conflict. Still, Gaus says that Rawls's commitment to justice cannot sustain by itself without a "spirit of community" (p. 217). Gaus is right. But can community be sustained without a strong commitment to justice? Gaus's persons can find their identities only in a society that manifests jointly held values and concerns (p. 84), but a community of this type appears possible only if persons have respect for justice and uphold that commitment from a sense of duty. A too-concentrated focus on rights may all but disregard the importance of a duty to justice, and ultimately make impossible an environment that supports modern liberalism's conception of human nature.

STEVEN M. DELUE

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Weber, the Ideal Type, and Contemporary Social Theory. By Susan Hekman. (Notre Dame, Ind.: University of Notre Dame Press, 1983. Pp. x + 213. $19.95.)

Susan Hekman addresses one of the most important issues facing contemporary social and political theory, that is, despite the fact that the social sciences "have discarded the positivist foundations of their discipline . . . they fail to agree upon a viable replacement" (pp. 193-194). The dilemma is due to the fact that both of the major alternatives to positivist social science, interpretive theory and structural analysis (which Hekman labels the subjectivist and objectivist critiques respectively), contain insights into the nature of social reality and social theory that cannot be ignored, but neither is able to accommodate the insights of the other.

Given the respective strengths of the interpretive (of which she considers Schutz's phenomonology and Winch's version of interpretive theory) and structural analysis (Althusserian structuralism and Habermas's critical theory), any "successful postpositivist synthesis must supply a conceptual apparatus which can bridge the gap between interpretive and structural analysis" (p. 193). This problem can be resolved, argues Hekman, through a reconsideration of Weber's concept of the ideal type. After correcting some misunderstandings of Weber's ideal type, Hekman examines the various ways that Weber deploys it. She argues that because Weber's ideal type "offers a unified conceptual approach to analysis of both meaning and structural forms" (p. 60), primarily by establishing "a continuum between subjective meaning on the one hand and social structure on the other" (p. 54), "Weber's concept of the ideal type points to a solution of this problem" (p. 193) of a synthesis.

The philosophical and epistemological status of Weber's ideal type is, however, underdeveloped, argues Hekman. To provide a foundation for the ideal type, she turns to the work of Habermas, Toulmin, and Wittgenstein. Wittgenstein's account of the intersubjective nature of science (p. 175) and Toulmin's argument that there are standards of reason in addition to the rules of logic that are "field dependent" (p. 174) combine to provide the objectivity of the ideal type without having to claim the absolutist standpoint that positivism aspired to.

Hekman's argument is thoughtful and wide-ranging. Nonetheless, the book is marred by inconsistencies and omissions that detract from her account of the contributions and insights of the competing approaches she examines. And this in turn jeopardizes her ability to define the parameters within which the synthesis that she calls for will take place. In this respect the most important line of criticism is that her appreciation of the Wittgensteinian perspective is underdeveloped and confusing. She begins by limiting her discussion of the Wittgensteinian alternative (primarily in the work of Peter Winch) to ordinary language philosophy. To this approach she attributes what amounts to an underlaborer role (p. 88; the term underlaborer is Winch's) that Winch and Wittgenstein explicitly reject. She quotes Hannah Pitkin in support of this view of the contribution of Wittgenstein (pp. 88-89), ignoring the fact that Pitkin immediately goes on to characterize this interpretation of Wittgenstein's contribution as an impoverishment that trivializes Wittgenstein's perspective. Moreover, by focusing on ordinary language philosophy (and thus accepting the positivist appreciation of the Wittgensteinian project in social theory) Hekman ignores the work of political theorists such as Charles Taylor and William Connolly, both indebted to Wittgenstein and Winch, who have attempted to develop an interpretive social science which has the critical-structural dimension that Hekman quite rightly insists is necessary. She also claims that Wittgenstein and "Ordinary language philosophers . . . seal off the realm of mental states by labeling it 'unintelligible'" (p. 83). This is at best a caricature of Wittgenstein and has to ignore virtually all the work of Stuart Hampshire on the philosophy of mind and the relationship between thought and action. What is particularly odd about this is that Hekman's discussion of Wittgenstein in the last chapter seems to indicate a deeper appreciation than at first seems to be the case and belies her earlier characterization of Wittgenstein's potential contribution. But why she did not pursue the development of that potential in the more recent work of interpretive theorists is puzzling.

A similar pattern emerges in Hekman's discussion of Habermas. In the section on critical theory she focuses almost exclusively on Habermas's earlier work. She chides Habermas for failing to "offer an epistemological justification for the Archimedean point he accords to the analysis of the social theorist" (p. 144), ignoring the fact that Habermas (in his debate with Gadamer) explicitly denies that such a standpoint is possible for social theory. When Hekman finally does examine the more recent work of Habermas, she dismisses it too quickly and without pursuing the possible connections between Habermas's "universal pragmatics" and the interpretivist notion of intersubjectivity in detail.

To have considered the Wittgensteinian potential in more detail, to have pursued its development in the more recent work of interpretive theory, and to have examined the potential connections between interpretive and critical theory, might have enabled Hekman to more clearly specify the contours of the synthesis that she thinks Weber's concept of the ideal type can provide. Nevertheless, the book is at least partially successful: Hekman has undoubtedly made a case for the reexamination of Weber's concept of the ideal type, and in that respect her work heralds, I think, a possible new direction in social theory.

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The Ethics of Legal Coercion. By John D. Hodson. (Dordrecht: D. Reidel Publishing and Hingham, Mass.: Kluwer Academic Publishers, 1983. Pp. xiv + 174. $34.95.)

Philosophical analyses abound of the limits of law as an instrument of public control of individual action. This inquiry is noteworthy not only for its clear organization and expression but also for its nonutilitarian perspective. Justification of public power must flow from ethical principles that recognize "the supreme moral importance of each person's having control over his or her own life in accordance with his or her own unencumbered choices" (p. 13).

This basic principle of respect derives from Kant's imperative that humanity be treated as an end and not a means. A person's choices must be respected unless there is a relevant encumbrance upon the person's capacity for voluntary, informed choice. Coercion that intrudes upon such choice must be founded upon legitimate, affirmative justification and not prohibited by independent libertarian principle. Coercion is not disrespectful of persons when it serves goals expressed by these principles: *harm:* to prevent personal action which violates the protected spheres of others; *paternalism:* to effectuate the choice a person would make if unencumbered; *welfare:* to require persons to bear a reasonable burden to provide vital aid to others; and *community:* to provide fair collective control of other-regarding common decisions. Further, extra-inclusive requirements are proper only if necessary to attain these aims and if the burden is fairly distributed and does not impose a major sacrifice on anyone.

Positive justification is not sufficient, however, to meet objections which transcend the claim of coercion. Democracy and free speech, for example, are special social arrangements required to give meaning to respect for personal participation in collective decision making. Interference with these expressions of respect must not only be founded upon pursuit of legitimate, affirmative goals but must also be defensible on grounds which are sensitive to the specific and extraordinary importance of these arrangements.

The model is then applied to several problems. Consequentialist theories of punishment must be placed within the framework of the respect principle. The guilt-innocence distinction—rooted in the offender's knowing choice to violate a legitimate legal command—must be renewed; proportionality becomes critical in assessing severity. Because the concept of public goods is not coextensive with the affirmative justifications, there is no categorical justification for taxation for the provision of public goods. Likewise, characterizing pornography as a victimless crime begs the question: the necessary link between the activity and harm. Finally, the prohibition of offensive conduct can only be justified, if at all, through application of the community principle.

This work is well-conceived, coherently organized, and clearly written. The analysis could be enhanced significantly by research in relevant case law and legal scholarship. The argument regarding the principle of necessary means seems facile to one familiar with the development of the necessary and proper clause in U.S. constitutional law. The section on free speech could benefit from a more comprehensive examination and use of the extensive and complex legal developments. Although the cultural dimension of language is often noted, certain unexamined presumptions are built into the language. Our individualistic mode of thought allows Kant's imperative regarding the treatment of "humanity" to be restated in terms of "human beings" without explicit attention to the difference.

Nevertheless, the work constitutes an important beginning to a process of slipping between the horns of a modern dilemma: to be a moralist for whom there is only a morality of ends and not of means or to be an economic libertarian for whom there is only a rationality of means and not of ends. Both models treat persons as means in service to some utilitarian or organic conception of the good of humanity. It is hoped that herein begins a new approach: the independent justification of means and ends from an integrated ethical perspective. Good ends do not justify the means used; fair means do not justify ends produced. Justice inheres in good ends pursued through fair means.

JAMES E. LENNERTZ

Lafayette College

The Future of American Democracy: Views from the Left. Edited by Mark E. Kann. (Philadelphia: Temple University Press, 1983. Pp. xiv + 305. $29.95, cloth; $9.95, paper.)

Originating from a conference held in 1981, this collection of 13 interesting and well-written essays about the future of American democracy reflects a significant trend of the past decade: the extension of the purview of democratic thought and action from the formal political institutions of society—city councils, state legislatures, the federal government, and the politics of politicians—to the entire range of human associations within a given society. Mark Kann speaks for all the contributors, who include politicians as well as academics from a variety of disciplines, when he identifies democracy as "a comprehensive way for people to speak together, participate together, and make decisions together. Democracy thus concerns politics, economics, discourse, science, community and family life, and more" (p. 9). This definition recognizes the many sources of democracy, not all of which are expressly political and not many of which are fully developed by pursuing the liberal cause of equal opportunity.

As "leftists," the authors represented in this volume see themselves as democrats first. Although many sympathize with Marxist analyses, socialist goals are subordinated to democratic means and ends. Indeed, Kann, Fred Harris, and Peter Bachrach believe that their views are the only authentically democratic ones. They charge that, despite the use of the rhetoric of democracy by the dominant interests and politicians of contemporary American society, the current power structure impairs the ability of citizens to participate as equals in the control of their lives, impoverishes rather than enriches the *demos*. Although the political system more or less preserves the rights and privileges of citizens, the majority in America today is really impotent. Those who rule are elites, detached from and unrepresentative of "the people" who would rule.

According to Kann, the contributors to this volume envision their task to be unmasking the inequalities that constrain the power of the *demos* and suggesting ways for their eradication. Underlying this approach is the Rousseauian view that current social inequalities do not stem from the inadequacies or diversity of human nature, or from an appropriate deference to expertise, but from the interests of established elites in maintaining their status by means of domination and subordination.

In line with Kann's definition of democracy, the essays cover a wide range of territory. Kann, Harris, Bachrach, and Murray Edelman map the justification for the left-democratic perspective at a general level. The other essays are more focused and do not merely concern "democracy and *x*." They include: a fascinating interpretation of the politically value-laden nature of primate research (Donna Haraway); telling but ordinary analyses of United States' foreign policy generally, and with regard to South Africa in particular, that emphasize the interests of American capital and thoughtless hostility towards the Soviet Union as its most important determinants (Edward Friedman and Bernard Magubane); a deconstruction of "the family" based on an analysis of the mutual effects of the division of labor within the family, the economy, a society as a whole that suggests the possibility of new forms of healthy reciprocity between the sexes (Jane Flax); a useful catalogue of the devastation wrought by the Reagan administration's tax and budgetary policies on the poor in particular, and the economy in general, that points out why large budget deficits should be opposed by leftists (Robert Alford); a critique of the bias of the American economy towards "monopoly, militarism, and multinationalization" (p. 116) that imagines its replacement by applying Aristotle's ideal of integrating private possession with public use through the democratization of investment (Maurice Zeitlin); slightly musty musings on the need for Americans to define their "frontiers" in terms of quality and self-development rather than materialism and expansionism (Tom Hayden); and useful documentary histories of the manner in which scientific research has been directed by the needs of private enterprise and the military establishment rather than the unfettered life of the mind or the needs of society as a whole (Robert Kargon, Charles Schwartz).

The expanded domain for the interpretation of democracy in American politics and society, coupled to the politically engaged stance of the authors, makes this collection of essays exciting, but it also creates hazards, not all of which have been avoided. For example, some of the great value of the authors' perspective stems from its ability to disclose new aspects of specific activities by situating them in a broader context and/or by revealing the politically informed, nontechnical character of all forms of social life. This can be particularly illuminating as long as the diversity of human activities is not ignored, and their problems are not reduced, to those of politics and power. Some of the essays recognize this better than others. Second, although the authors have demonstrated important ways in which "the people" or "the public" have been frustrated by the inequalities of the political system, they sometimes are too loose in their referents, in which case the unhappy "people" and their problems are no longer identifiably American. The *demos* has been reified. Maybe Reagan doesn't represent the best of the American people, but he did win in 1980, and he may win again.

John R. Wallach

Yale University

Men and Citizens in the Theory of International Relations. By Andrew Linklater. (New York: St. Martin's Press, 1982. Pp. xii + 232. $22.50.)

The current dearth of books written on the theory of international relations makes this title especially welcome. Composed as a doctoral dissertation at the London School of Economics, Linklater's book is inspired by the work of what was once called the "British Committee on the Theory of International Relations," who took as their point of departure Martin Wight's definition of international theory as "a tradition of speculation about relations between states [and] a tradition imagined as the twin of speculation about the state to which the name 'political theory' is appropriated" (*Diplomatic Investigations,* George Allen & Unwin, 1966, p. 17). Linklater approaches, as the British Committee did, one of the long-standing problems of international theory—that is, the dichotomy of obligations for "man *qua* man" and "man *qua* citizen"—with a complete historical account of the philosophy that has addressed the problem and a unique formulation of his own.

The essence of Linklater's thesis is that the distinction maintained in modern international theory between mankind/ethical universality/potency on one hand, and civil society/ethical particularity/actuality on the other, may be overcome with a historical critique that synthesizes social contract and universal morality by refounding international theory in a nonrational ethical source: the historical development of human freedom. The thesis is developed through a detailed and thoughtful exposition of the relevant work of the major Enlightenment jurists including Kant, Hegel, Gentili, Pufendorf, and Vattel, with occasional references to the sociological perspectives of Durkheim, Weber, and the early Marx.

The historical exegesis is followed by an attempt to "seek a non-rationalist foundation for the traditional belief in obligations to humanity, and for the recovery of the critique of the international states-system" (pp. x-xi). What makes Linklater's thesis most interesting is that he strives to this theoretical goal while still asserting the self-determining, self-developing character of individual freedom. That is, he implies a theory by which the nation-state may be transcended without subordinating the liberty of individual citizens to his project.

This is accomplished at the expense of both contractual theory and natural right theory. Although Linklater's critique is persuasive, the reader is not wholly certain that he has not merely uncovered again a fundamental question rather than resolved one. His efforts to grapple with the problem are admirable and at times exciting, but his conclusions are less convincing (by traditional logic, at times even incomprehensible) than those of the philosophical constructions on which he has expended the bulk of his critique.

If, as Linklater claims, we are able to transcend the rationalist source of freedom and find its particulars in the historical development of freedom, then in what historical epoch are these particulars self-evident and thereby realized? In reaffirming Kant, and here the later Marx, on the viability of an international system based on individuals confronting each other face to face as human beings rather than as members of particular subgroups of humanity, Linklater has dismissed as an abstraction Hegel's tenet that subjective development is concomitant with objective development.

Unfortunate as it may be, the Hobbesean description of world politics still seems to be a more accurate representation of current events than the Kantian one. As Wight wrote: "International theory is the theory of survival. What for political theory is the extreme case (as revolution, or civil war) is for international theory the regular case." Short of a present eschatological development, the historical epoch of realized individual and civil freedom does not seem imminent, and therefore Hegel's *Phenomenology* remains a more convincing description of the development of freedom than Linklater's "scale of types" drawn from Marx's principles of historical periodization.

Linklater appears at odds with his dismissal of Hegel when he admits that his conclusions lack "empirical support," and that he has not attempted to "suggest how the study of international relations might accommodate the study of human freedom" (p. 166). His purpose is merely to put forth "various ideal types of international relations," and this is perhaps justification enough for the work. The usefulness of his exercise remains undiminished by the ideal nature of his conclusions for, turning Hegel to the defense of his critic, "The owl of Minerva spreads her wings with the falling of the dusk."

ROBERT E. KIERNAN

Fletcher School of Law and Diplomacy

Democratic Socialism: Theory and Practice. By Mihailo Markovic. (New York: St. Martin's Press, 1982. Pp. xvi + 215. $25.00.)

It has sometimes been maintained that Marxism necessarily resists any system of positive political theory. One reason for this view is that Marx himself seems in the Eleventh Thesis on Feuerbach to reject theorizing in favor of praxis. Further, it is said that because Marxism originates negatively as a critical theory of capitalism, it is inherently incapable of producing an affirmative theoretical program. Admittedly, Marx himself was wary of pure theory, as a result of which his modern followers have postponed the statement of creative and concrete theoretical proposals to an indefinite future. Thankfully, Mihailo Markovic's book does not accept such an anti-theoretical version of Marxism, so that he both breaks new ground in Marxist literature and offers an original contribution to contemporary political debate generally.

Markovic's philosophical point of departure is the humanist interpretation of Marx's *corpus* that he espoused in earlier writings. A single chapter suffices to summarize the doctrine of Marxist humanism. In the remaining eight chapters, Markovic endeavours to translate this humanism into a practicable political theory based on real possibilities for social change. Markovic does not limit his recommendations to the capitalist West; his insights are clearly intended to be of equal relevance to failed Eastern bloc socialism. Indeed, Markovic's closing chapter constitutes an incisive evaluation of the potential for genuinely revolutionary democratic reform in various East European nations, especially Poland.

Substantively, Markovic advocates a radical decentralization of political, economic, social, and cultural functions. This decentralization, stemming from his critique of bureaucratic centrism, is grounded by Markovic in a complex system of "federalism" (which closely resembles a notion of "confederation"). In contrast to bureaucratic structures, Markovic's federalism locates the totality of powers over the organization of all aspects of human life in localized participatory self-government. Both as individuals and as a community, people become directly and immediately responsible for their own condition. To deal with the inevitable conflicts between these basic units of self-control, Markovic posits several levels of democratically elected magistrates whose primary function is to arrange compromise leading to consensus. The exercise of authority beyond the basic units of democratic socialist self-government, so far from establishing itself autonomously, remains continually accountable to its electorate. In this sense, Markovic claims that his theory represents the true actualization of popular sovereignty: "The original and ultimate power lies in the people . . . members of a community . . . have the right to decide how much order and what kind of order they wish to have, which social activities and social functions should be supervised, planned and co-ordinated and to what degree of power" (p. 134). In the past, this viewpoint has been criticized as impractical. But Markovic addresses the procedural dilemmas that might interfere with the implementation of decentralized socialist self-government. Markovic thereby weds the theory of Marxist humanism to the practice of democratic socialism.

Much of the resistance to democratic socialism Markovic attributes to the single hegemonic conception of rationality shared by East and West alike. This dominant rationality, confined to the technical matching of means to ends, leaves untouched and enshrouds the ends themselves, and even implicitly denies that ends are subject to rational evaluation at all. Such instrumental rationality, Markovic recognizes, must be supplanted by a novel principle of reason suited to the peculiarities of democratic socialism. Unfortunately, Markovic's analysis of this "socialist" or "Marxist" rationality lacks detail, raising speculation as to whether a "rationality of emancipation" is a prerequisite or a consequence of revolutionary reform. Surely, the transformation of the theory of democratic socialism into practice demands identification of the salient features of this liberating reason.

CARY J. NEDERMAN

University of Alberta

Toward a Humanistic Science of Politics: Essays in Honor of Francis Dunham Wormuth. Edited by Dalmas H. Nelson and Richard L. Sklar. (Lanham, Md.: University Press of America, 1983. Pp. xiv + 615. $36.50, cloth; $22.75, paper.)

A volume that is well over 600 pages long and contains 20 articles all by different authors presents its readers, reviewers, and editors with a serious problem of unity and coherence. The problem is compounded in this case because the integrity of the volume rests on two different purposes: to honor the scholar Francis Dunham Wormuth and to advance the cause of a humanistic approach in political science.

The articles vary enormously in length, in style, and in content. Taken on its own terms, each is well-written. Many are intellectually challenging and even exciting. But, because of the great variety, a reader would be well-advised not to use a sample of one or two articles to judge the whole work.

The editors have carefully organized the articles into three unequal sections: political theory, problems of democracy, and imperial power. Each of the three sections is further divided into the categories that contain specific articles. Thus, on the surface the volume demonstrates a tight and careful organization.

The primary problem arises as one reads through the work and tries to make sense of how it all hangs together. A volume of this sort requires an outstanding editorial effort to create an overall rationale and to demonstrate how each article fits into it. If the editors were to claim only that the volume presented a series of articles by the students and friends of Professor Wormuth, then the reader would have no problem with the rationale and the significance of the work. But they claim it also represents an alternative approach to the study of politics.

This "humanistic" approach, they tell us, entails two things in contrast with "scientific methods" and the past "behavioral movement" (p. 575). They are (1) a problem orientation "critical of existing institutions" and "just as critical of movements that seek to change institutions" and (2) "an explicitly ethical standpoint" (p. 3).

It is, however, hard to see either this explicit standpoint in articles such as the one on "An Internation Power and Influence System" or the editors' providing a hint as to the connection between an ethical standpoint and the article. Even more difficult to understand is why articles such as "A Non-Mathematical Behavioral Model for the Analysis of Electoral Behavior" are not part of the non-"humanistic" "behavioral movement."

In the end, the editors fail to provide theoretical coherence to, or a coherent theory for, the work so that even their concluding modest claim that "the essays in this volume are united by their humanistic preoccupations" (p. 575) rings hollow.

It would be enormously unfair both to the editors of the whole volume and to the authors of the individual articles to imply that the absence of a coherent theory justifying the work's claim to theoretical significance thereby renders it valueless. On the contrary, most of the essays are of the highest quality and value. The articles contributed by the editors themselves rank among the very best and most important in the volume, and Nelson's article on the "privilege doctrine" in constitutional law could stand as a most important monograph on its own. In fact, the problem seems to be that the articles are rich enough to constitute three or four books. One of them, with substantially more development of theory, could have been the book described in the Introduction and the Conclusion that advances the cause of an alternative approach to the science of politics.

This valuable and interesting volume is particularly significant in illustrating the wide range of work being done from a great variety of perspectives that, for different reasons, distinguish themselves from the recent mainstream in political science.

RONALD E. PUHEK

Michigan State University

Liberal Democracy: Nomos XXV. Edited by J. Roland Pennock and John W. Chapman. (New York: New York University Press, 1983. Pp. xv + 453. $37.50.)

Liberal Democracy is the twenty-fifth volume of the *Nomos* series published by the American Society for Political and Legal Philosophy. The book contains essays by 17 legal scholars, political scientists, and philosophers on various issues concerning liberal democracy: representation, federalism, judicial review, freedom of speech, and other topics. Some of the essays were presented at the annual meetings of the Society in December, 1980 in conjunction with those of the American Philosophical Association.

As to be expected with a volume of invited essays, the quality is mixed. Some are very good, others would not be accepted (one would hope) by refereed journals. The general impression created by the volume is that democratic theory, in contrast to, say, public choice theory or to the theory of distributive justice, has not progressed very much in recent times.

Stephen Darwall and Charles Beitz have interesting essays on representation and procedural equality respectively. In a short comment on an essay by Robert Dahl, David Braybrooke raises some important questions about the compatibility of liberalism, democracy, and federalism stemming from Amartya Sen's "Liberal Paradox." Frederick Schauer's essay critically examines defenses of free speech based on the value of democracy.

The best chapters, in my view, are Peter Railton's "Judicial Review, Elites, and Liberal Democracy" and Kenneth Winston's "Toward a Liberal Conception of Legislation." Railton's essay, originally a comment on a paper by Robert McKay, is a sustained and persuasive attack on the view of the United States Supreme Court as a bulwark against illiberal or undemocratic tendencies of other branches of government. Although I am sympathetic to the bulwark thesis, were I to decide the issue solely on the basis of the essays on judicial review in this volume, Railton's essay would without doubt carry the day. Kenneth Winston worries about the liberal character of legislative policymaking, noting that this concern does not appear to be shared by democratic theorists. He examines the various ways generality of legislation in liberal theory is intended, but fails to secure liberal legislation.

As a whole, *Liberal Democracy* does not break any new ground. The essays by Railton, Winston, and some others are certainly worth studying, but the quality of the papers is too uneven to make the volume useful in, for example, a seminar on democratic theory.

CHRISTOPHER W. MORRIS

University of California, Los Angeles

Marxism: Nomos XXVI. Edited by J. Roland Pennock and John W. Chapman. (New York: New York University Press, 1983. Pp. xv + 342. $35.00.)

Marxism: Nomos XXVI contains 12 essays divided into four sections dealing with Marxism and morality, politics, law, and other "unresolved issues."

In the lead essay, Richard Miller argues that Marxism represents "a radical departure from the moral point of view" (p. 4). Marx's views, like those of Nietzsche and Weber, are "amoral" because they reject the principle of disinterest or neutrality which distinguishes the moral outlook from others based on class interest, nationality, or aesthetic concerns. In his response, Patrick Riley wonders whether Marxism may not contain a morality whose central features are "love" and "decency" (p. 43). For Riley, Marx's discussion of the "realm of freedom" at the end of history is the political corollary to Kant's injunction to treat individuals as "ends in themselves," that is, with equal respect and dignity. Socialism is not so much produced by historical causes, but must be chosen on the basis of our power of practical reason which has an independent status apart from the process of history. Likewise, Frederick Whelan notes that Marxism admires certain character traits such as "moral heroism" and "heroic virtue" exhibited by the workers in their struggle for socialism (p. 61). Marxism represents a theory of the virtues built upon a conception of revolutionary heroism which, Whelan admits, may not be morality in Miller's deontological sense, but which has direct linkages to the civic humanism of earlier republican traditions (pp. 67-68).

Perhaps the most controversial essay is Sheldon

Wolin's "On Reading Marx Politically," the thesis of which is that "Marx developed a theory that furnished strong grounds for the perpetuation rather than the revolutionary overthrow of capitalism" (p. 84). Marx's "theoretic failure" consists in the fact that he remained wedded to an outmoded faith in revolutionary praxis and struggle that was at odds with his deeper insights into the tendency of modern capitalism to reproduce its conditions of existence. The prediction of revolution, then, becomes a kind of "solace" in the face of the overwhelming power of the modern forces of science and technology (p. 107). This reading is attacked first by Stephen Holmes, who accuses Wolin of indulging a penchant for a "romantic" identification of virtue with poverty, and later by Alan Gilbert, who regards Marx's life-long revolutionary commitments as evidence against Wolin's severence of Marx's theory from his political practice.

In the discussion of Marxism and law Mark Tushnet finds it difficult to conceive a theory of law that is both true and "distinctively Marxist." His conclusion is that Marxism is more a "statement of affiliation" than a distinct theoretical position (p. 185). Leon Lipson counters with the claim that the "fundamental characteristic" of Marx's legal theory is its attempt to examine law from the "outside," namely from the standpoint of politics and economics. And Tom Gerety argues that the lawyerly practices of fair dealing and due process are incompatible with the Marxist commitment to revolutionary struggle.

In the concluding section of the volume G.A. Cohen expresses certain reservations about the truth of historical materialism. He now believes that while his previous "defense" of Marxism was correct as an interpretation, that Marxism may still be false. Marx, he argues, went too far in a "materialist direction," giving too much explanatory weight to the activity of production and not enough to forces like religion and nationalism (pp. 238-240). Also, Marx's concern to abolish "social roles" and the division of labor fails to take account of how the constraints imposed by these institutions may help to foster a "satisfying community" (p. 236). Peter Stillman offers a fine commentary on the meaning of "critique" in Marx's enterprise and along the way offers convincing reasons why Marxism should be regarded as an "interpretive" rather than a reifying or positivistic theory. Finally, Jon Elster reflects on the meaning of exploitation and economic injustice in Marxism. Elster argues against Marx that workers are not coerced into selling or "alienating" their labor power and against liberals that free market transactions may yet generate exploitative social relations.

With the possible exception of Elster's chapter, the most conspicuous absence in this volume is any discussion of Marxism and economics. Marxism for the scholars represented here has become almost exclusively a theory of "superstructures" with no clear account of how these are related to the economic "base" of society. An essay attempting to explain how Marxism has arrived at this curious state of affairs might have served as a useful introduction or conclusion. These reservations notwithstanding, *Marxism: Nomos XXVI* is a welcome addition to recent critical studies of Marx and Marxism. Only its prohibitive cost will prevent it from reaching the audience it deserves.

STEVEN B. SMITH

Yale University

An Introduction to Plato's Laws. By R.F. Stalley. (Indianapolis: Hackett Publishing, 1983. Pp. x + 208. $18.50.)

In recent years much of the best work in classical scholarship has been done by philosophers applying sophisticated analytical techniques and publishing their results in philosophy journals. In the study of Plato, progress has been especially apparent in metaphysics and epistemology, but significant advances have also been made in moral and political philosophy. The need to transmit these results to a wider audience has become apparent, and so R. F. Stalley's philosophical introduction to the *Laws* is a welcome addition to the literature.

Stalley, Lecturer on Moral Philosophy at the University of Glasgow, concentrates on "those aspects of the *Laws* that have most significance for the problems of philosophy as we now understand them" (p. ix). He is also concerned with elucidating the overall structure of the work (p. 4) and with calling greater attention to the *Laws:* "if we are interested in Plato's political theory—that is, in his answers to what we now understand as political problems—it makes sense to regard the *Laws* as the primary source while treating the *Republic* as a kind of prolegomenon designed to set out the underlying principles" (p. 22).

Stalley provides general background in two brief introductory chapters. Subsequent discussion is organized according to topics, although it roughly parallels the order of exposition in the *Laws* itself. Some representative topics are: "The Nature of Law," "Pleasure and the Good Life," "Society," "The System of Government," "Education and the Arts," "Punishment," "Responsibility," and "Religion." Each chapter is headed by a useful list of the passages in the *Laws* and some other Platonic works most relevant to the discussion.

The book's greatest strength is that it largely accomplishes its aim. The important topics in the *Laws* are analyzed lucidly, and the overall coherence of the work is brought out. Stalley combines high standards of scholarship, philosophical argument, and judiciousness. This is clearly exhibited in his discussion of moral responsibility (chap. 14). In Book IX of the *Laws* (859c-64b), Plato presents a difficult and obscure attempt to reconcile the Socratic doctrine that all wrongdoing is involuntary, to which he still adheres, and the obvious need to hold wrongdoers accountable before the law. Stalley lays out the central difficulties, presents and criticizes the major attempts to deal with them in the scholarly literature, and then concludes that satisfactory solutions are not readily forthcoming. Stalley should be commended for elucidating what is actually in Plato's text, for not attempting to make the *Laws* more coherent than it is.

We can probably credit Stalley's philosophical bent for his book's most serious shortcoming, his failure to discuss various matters of political theory adequately. Stalley's valuable accounts of such topics as punishment and moral responsibility receive 14 and 15 pages respectively. Plato's system of government receives 11, and this includes discussion of the nature of the mixed constitution and the extent to which the political system is democratic. Stalley includes a useful six-page appendix on the constitutional structure of the state, but important political questions receive scant attention. For example, Stalley presents some suggestive remarks connecting the development of Plato's political thought from the *Republic* to the *Laws* with other changes in his philosophical system. But these are only remarks, receiving roughly two paragraphs (pp. 21-22). The multitudinous puzzles and anomalies in the *Laws* system of government are almost undiscussed. Thus, the vexing question of reconciling the nocturnal council and the auditors introduced in Book XII with the bodies of officials described in the earlier Books receives essentially a single paragraph. Stalley's lack of attention to these and other political topics is surprising. Had he discussed them in more detail, he would have written a more valuable introduction to the *Laws* for readers interested in Plato's political theory.

GEORGE KLOSKO

University of Virginia

Alternative America: Henry George, Edward Bellamy, Henry Demarest Lloyd and the Adversary Tradition. By John L. Thomas. (Cambridge, Mass.: Harvard University Press, 1983. Pp. vii + 391. $25.00.)

This book is about the lives of three authors of great importance to the period in which they wrote between the end of Reconstruction and the rise of Progressivism, the "adversary tradition" which they shared in shaping, and what is common to their visions of an "alternative America." As such, this book must be applauded as a sterling contribution to our understanding of what is particularly American about our political ideas as they express our perceptions and inform our reactions to what we see in the social world about us.

With respect to the men of this book's title, they are remembered best for the popular classic each wrote in response to the modernization crisis of the times. The order of their names is given by the order in which their great American books were published: *Progress and Poverty, Looking Backward,* and *Wealth against Commonwealth.* In three deftly interwoven strands of lucid and thoughtful biographical narrative, Thomas examines how each book came to be written and its subsequent significance not only for the political currents of the times but also for each author's own gravitation toward, and final commitment to, a life of redemptive political advocacy. Although Thomas gives us three lively portraits of sons of ardent churchmen finding their way into American politics through journalism and public speaking, he also pays careful attention to their ideas. Thus, at appropriate points in the narrative, a full chapter is devoted to a consideration of the structure and key ideas of the major work of each author-activist. In these chapters, Thomas shows that these men were not opposed to development as much as they were opposed to uneven development.

At the same time that this book may be read with profit as intellectual and political biography illuminating the relationship between writing and acting, it also contributes to our understanding of a distinctly American (New World-liberal) variety of oppositional politics that Thomas calls "the adversary tradition." Notwithstanding the great differences that distinguish their respective political nostrums, Thomas shows how George, Bellamy, and Lloyd contributed to a pietist expression of political protest that looked to a national change of heart in preference to class war as the solution to what ailed the new industrial capitalism. Thomas delineates the essential characteristics of this "adversary tradition" that sought to defend the providential republic of The Founders against Marxism on the Left and modernizing corporatism on the Right; relates this American, middle-way rejection of the wrong kind of growth to certain Jeffersonian, Protestant evangelical, and eighteenth-century producerist antecedents; and gives his views on the ambiguous nature of the "adversary tradition's" relationship to the Progressive movement that followed.

Thomas concludes by suggesting that the true legacy of these utopian author-activists is a vision of an "alternative America" in which regional planning and some government ownership combines with a high degree of localism and decentralization to form an arcadian order of even development whose natural characteristic is cooperative solidarity. Thomas comments in a brief but nonetheless persuasive fashion on the garden city visions of Frank Lloyd Wright and Lewis Mumford and on the TVA as twentieth-century expressions of the "alternative America" bequeathed to us by the "adversary tradition" of George, Bellamy and Lloyd. I only wish that Thomas might have continued his narrative in order to consider the brief revival of American populism in the 1960s within the rich and suggestive framework this book elucidates in a cogent and very readable way.

DARRYL BASKIN

Elmira College

Empirical Theory and Methodology

Approval Voting. By Steven Brams and Peter Fishburn. (Cambridge, Mass.: Birkhauser, 1983. Pp. xix + 198. $14.95, paper.)

Approval voting is an election procedure that allows each person to cast a vote for as many candidates as he or she wants. The voter is not restricted to one alternative, as in most currently popular election procedures. A number of people have recently independently "discovered" approval voting. But the authors of this volume have probably done the most, both to investigate it rigorously and to advocate it within political science and to a broader audience. The present book brings together the various aspects of the case in favor of approval voting.

The book divides into three parts that are loosely connected and not entirely consecutive. The first chapter is addressed to the widest possible audience and makes the case for approval voting using normative arguments that are likely to be both comprehensible and persuasive to such an audience. The remainder of the book is more technical. It consists, first, of investigations concerning the properties of approval voting and of comparisons of approval voting with selected alternative voting systems (e.g. plurality, plurality with runoff, and approval with runoff). But it consists also of careful and detailed analyses of a variety of real elections (among others, for House Majority Leader in 1976, U.S. senator from New York in 1970 and 1980, and U.S. president in 1968 and 1980). An attempt is made in these analyses to reconstruct the preferences of voters based on election results, polling information, and other sources. This allows for the estimation of likely results under alternative voting rules. It appears on this basis that approval voting would have favored Goodell over Buckley and Ottinger in New York in 1970, Holtzman over D'Amato and Javits in 1980, and would have dramatically increased the chances of Baker in the 1980 Republican primaries and of Anderson in the election.

The more technical part of the book (8 out of 10 chapters) follows closely other publications of the authors that have appeared in a variety of sources since 1978. The attempt is clearly made to strike a compromise between rigor and readability. The text includes definitions, assumptions, and theorems, but not the proofs (with few exceptions). For the proofs, the reader is referred to the various publications from which the chapters are derived.

One can quibble about the book's loose organization, too obviously determined by the availability of already written (and published) material. One can also wonder whether the compromise between rigor and readability really succeeds. Political reformers are likely to read only the first chapter and maybe skim the conclusions of the others. Political scientists and social choice theorists are likely to miss the proofs. The *book*, in short, has its faults. But the research project it presents is a social scientist's dream. It is a combination of deductive mathematical theory, sophisticated and innovative data analysis, and obviously important suggestions for reform, a reform that is limited, realistic, and clearly (to me anyway) beneficial. What more could one want?

With the analysis and advocacy of approval voting, modern social choice theory enters an arena within normative political theory from which it has been absent, an arena within which serious proposals for concrete reform are discussed. If one can judge by the present book, both normative political theory and social choice theory will need to change to accommodate this innovation. The case for approval voting made here consists of two sets of arguments, each showing little sign of internal theoretical coherence, and both almost entirely unrelated to each other. First, for the general audience: Approval voting gives more flexible options, would elect the strongest candidate, would give minority candidates their proper due, will add legitimacy to the outcome, and would increase voter turnout. Then, for the social choice theorist: Compared to plurality voting and to a few other alternatives approval voting is more sincere and strategy-proof, it is more likely to elect a Condorcet winner, it is more equitable (on one criterion of equity), and makes the average voter more efficacious. The two sets of arguments overlap little, even allowing for the looseness of translation. Furthermore, each set is not much more than a list. What is missing here is a unified, coherent theory on the basis of which the case for approval voting could be made, a theory within which the arguments for a general audience really would be simplified verbal forms of the more technical arguments.

KAROL SOLTAN

University of Maryland

Foundations of Social and Political Processes: The Dynamics of Human Behaviour, Politics, and Society: Vol. 1, **Theory.** By Johan K. De Vree. (Bilthoven, Netherlands: Prime Press, 1982. Pp. xiii + 446. Dfl. 58.00.)

One of the interesting realities of the stratification system in contemporary political science and sociology is that although the work of most scholars has little relationship to grand or even middle-range theory, those who produce such theories frequently receive the greatest disciplinary rewards. The fact that there is little agreement about the nature of theory does not detract from an understanding that there is something special about its development. The more encompassing and extreme the theory, the more valuable the theorist. It is in the context of these comments, and on the basis of *Foundations of Social and Political Processes*, that I suggest that Johan K. De Vree is an extremely valuable member of the political science discipline.

This volume, the first of two, "describes, in an informal manner, the central elements of a general theory of human behaviour and social life" (p. i). The theory is a general theory, a grand theory, because it provides the tools for an understanding of human action and interaction "in all their variegated forms" (p. i). A highly formalized theory and associated issues of operationalization and measurement will be addressed in Volume 2. Although De Vree recognizes that this project smacks of hubris and intellectual arrogance (his terms), a more restricted and modest theory would fail to reflect the interdependent nature of social reality. De Vree's earlier work on international organization and integration led him to the realization that common distinctions such as that between international and domestic affairs or that between academic disciplines, as well as the ambiguous nature of natural language, impede rather than enhance our understanding.

The book is divided into two parts comprising 13 chapters. In addition there is a glossary and series of 58 diagrams illustrating various processes. Because De Vree is critical of the sloppy nature of the terminology employed in much social science discourse, it is particularly important for him to define his terms in an unambiguous manner. The glossary of approximately 100 terms provides easy access to definitions. It was, however, annoying to encounter a number of examples of the following nature: "Access: See Demand Access"; "Behavioural State: See Behaviour"; and "Equilibrium: See Social Equilibrium."

Part 1, "On Theory and Methodology," contains two chapters on the nature of theory in general and systems theory in particular. For De Vree, the growth of a science depends on the quality, precision, and scope of the theory on which it is based. Statistical research and the testing of isolated hypotheses are meaningless in isolation from general theory. In order to understand the complex interdependent nature of social reality, De Vree bases his analysis on a systems theory. Because a system, by definition, is composed of a set of interrelated elements, the traditional cause-and-effect language has limited applicability. The value of the systems approach lies in its recognition of the existence of complexity and interrelatedness and in its stress on the importance of feedback relationships. The approach also makes evident the folly of assuming that social phenomena can be studied according to organizational and disciplinary boundaries. The guiding principle of the approach is that it explains "the conduct of any individual, group, subsystem, or aspect of behaviour, in the context of, and as a function of, the configuration of which it is a part" (p. 25). One problem with many systems models is that they fail to provide any guidance for intervention and social change.

Although the provision of such guidance is not one of De Vree's goals, many members of his audience will be looking for such guidance.

Part 2, "Behaviour, Process, and System Development," contains the description and application of the theory. De Vree distinguishes two sorts of systems: atomic and compound. The former (individuals, for example) are assumed not to be made up of subsystems, and the latter are assumed to be made of systems of atomic systems. De Vree moves from chapter to chapter, from atomic to compound systems, in a progressive manner. This movement is from a theory of behavior and the nature of interaction; to system capacity; to trade and cooperation; to violence, diplomacy, and investment; to regulation and institutionalization; to culture; and finally, to the evolution of society.

This is an impressive work. One basic question that I cannot answer is whether the quality of the work is due to the theory or to De Vree's insightful understanding of human behavior, politics, and society. Because I tend to believe that it is the latter, and I do not share De Vree's faith in formal mathematical theory, I am afraid that I shall not follow him to Volume 2. I am, however, grateful to him for this volume and recommend it to all political scientists.

MARK OROMANER

Hudson County Community College

Reason in Human Affairs. By Herbert A. Simon. (Stanford, Calif.: Stanford University Press, 1983. Pp. viii + 115. $10.00.)

This slender volume contains three lectures delivered in 1983 at Stanford University. In lecture 1, Simon sketches three conceptions of rationality, three models of reasoned decision. The first, labeled "subjective expected utility theory," is an overarching formal structure in economics cited as "one of the most impressive intellectual achievements . . ." of the century (p. 12), although it is "impossible to employ in any literal way in making actual human decisions" (p. 13). The second, "bounded rationality," is a psychology-based model of the way decisions are actually made: The actor focuses on a limited issue ignoring most options, limits the values brought into play, and spends time primarily gathering information. The requirements for such decisions are minimal: an actor able to focus attention narrowly, to generate alternatives, to acquire facts, and to infer from those facts. The third model, "intuitive rationality," is collapsed with "bounded rationality" and contributes little to the exposition.

Lecture 2 is an excursus into evolutionary mechanisms, very loosely related to the central theme. The major point to emerge is that self-interest provides the prime impetus for human action. Although self-interest can be "enlightened," that means only taking into account long-run as well as short-run benefits to the self.

Lecture 3 examines the factors that influence institutional rationality. Three prime limits are identified: human inability to deal simultaneously with a number of issues, which forces serial treatment of problems and causes faddishness and single-issue politics; the presence of multiple values in society, which requires acceptance of Arrow's social welfare theorem, that is, there is no solution of interest conflicts in majority-rule political systems; finally, the uncertainty of human knowledge rules out optimization, which Simon replaces by something called "satisficing," that is, settling for "good enough" rather than optimal. Institutional rationality is strengthened by instituting structures and processes that limit the information requirements of the decision maker, particularly organization, specialization, use of markets, and pricing. The limits on human capacity to deal with large systems can be extended by the use of technical decision tools (operations research and artificial intelligence systems). The most intransigent problem is the inadequacy of the information supply, exacerbated by the transitory character of media information and the difficulties encountered in legitimizing experts in controversial areas. Further, increased knowledge would assist with some types of human problems but contribute nothing to others.

Some of the observations and criticisms that appear in the lectures are highly suggestive, but their collective impact is vitiated by the palpable inadequacy of the underlying conception of knowledge, its acquisition and use—the epistemological base is faulty. Reason, for example, is very narrowly defined to coincide with formal logic or calculation (p. 5), but "rationality" is extended to include calculating "effective courses of action." That leaves an enormous gap in the conceptual structure, nowhere adequately filled or even articulated. When Simon asserts that "[reason] cannot tell us where to go but it can tell us how to get there" (p. 7), he is simply wrong. Formal calculation is necessary for developing and using *both* values and priorities; it is sufficient for neither purpose. The creative dimension of knowledge use (or acquisition), most simply illustrated by the process known as diagnosis, has been left out of the apparatus. Further, the central process of generalizing experience, of creating and justifying propositions whose content is greater than the set of descriptions from which they are presumably "induced," is glossed over. Yet these are the only type of knowledge structures from which inferences can be made—nothing can be inferred from a description.

The effects of epistemological inadequacy taint much of the discussion. Key analytic concepts are used improperly: Values are identified with goals (p. 11); theories are said to describe (p. 22); inferences are to be drawn from facts (p. 22). The stipulated requirements for rationality are redundant, an indication of inadequate analysis: Given the capacity to generate alternatives, the capacity to make inferences is already present. Finally, conclusions generated in other fields of inquiry, or derived from postulated (not induced) systems, are transferred to human affairs with a cavalier disregard for evidentiary requirements. Thus, the case against altruism is derived from genetics (p. 105), and the cause of political faddishness is found in "people's inability to think about a lot of things at once." But the transfer from genetics certainly requires additional evidence and the causal attribution rests on a simple fallacy—not separating instantaneous consideration of problems from sequential consideration during a relatively short time period, or simultaneous consideration by separate agents. The claim that markets (pp. 88-89) or adversary proceedings (pp. 90-91) increase the rationality of social institutions also requires evidence beyond inferences from earlier postulates. For these and other reasons that cannot be included in a brief review, the argument does not cumulate or convince. The lectures are only discussions.

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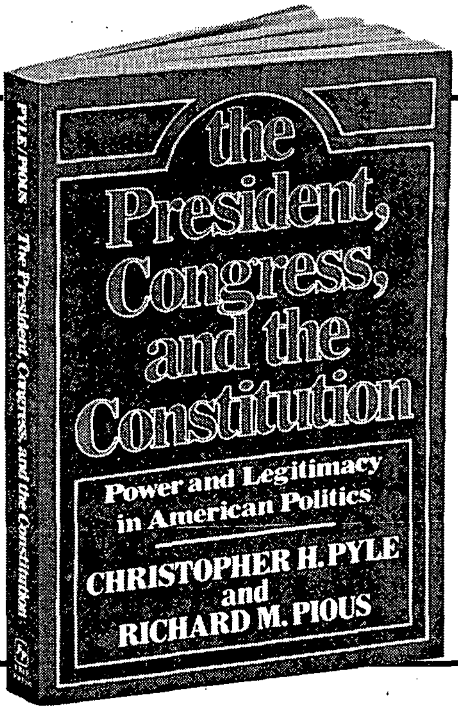
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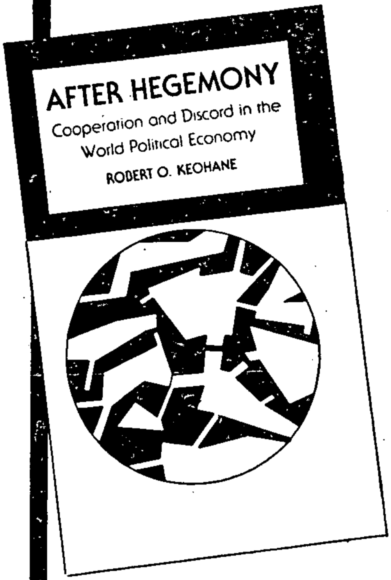

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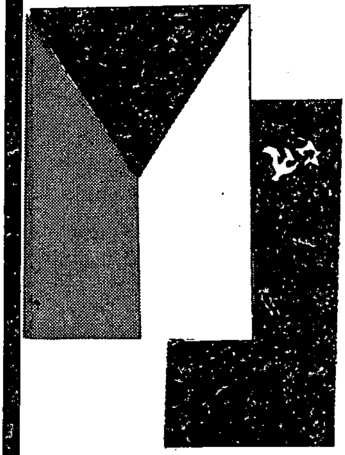

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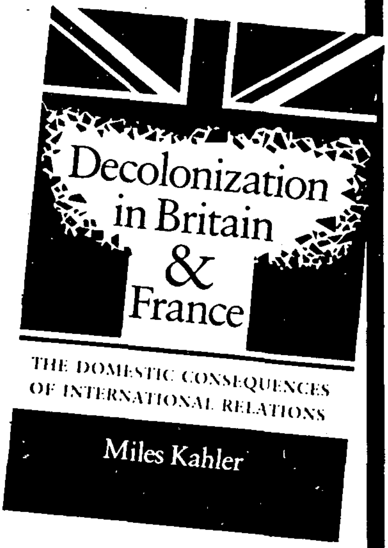
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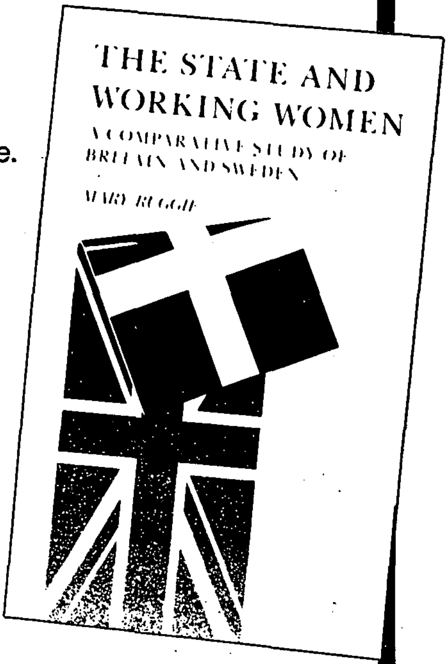
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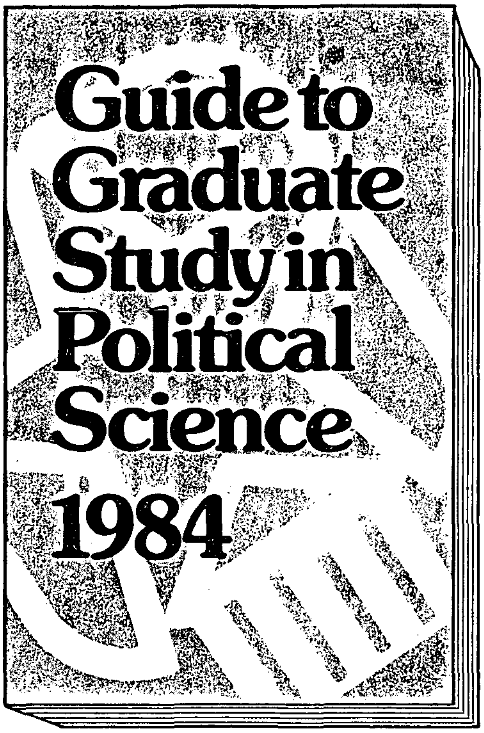
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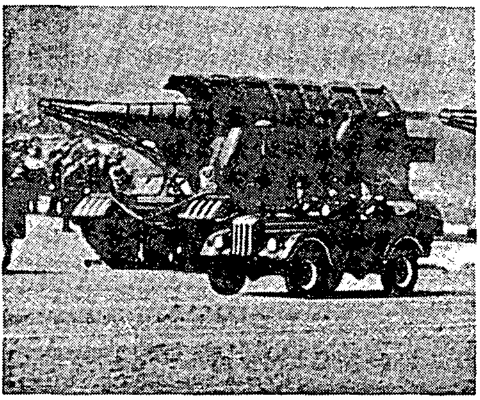

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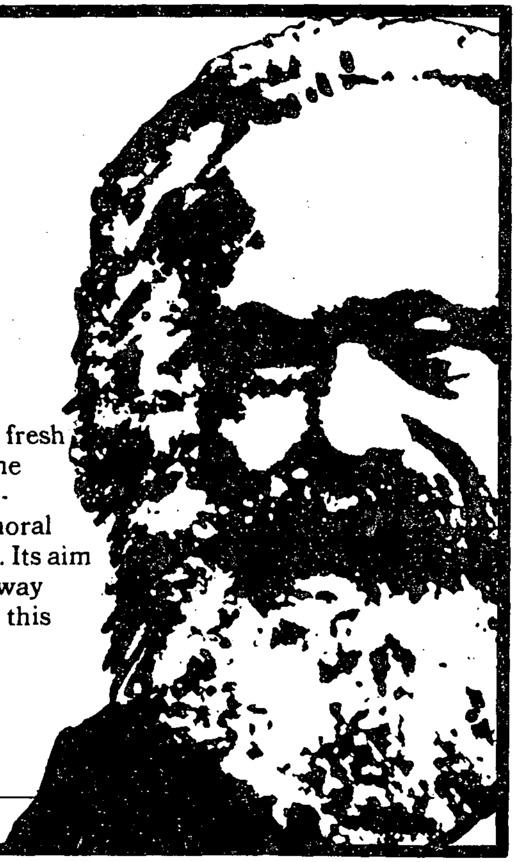

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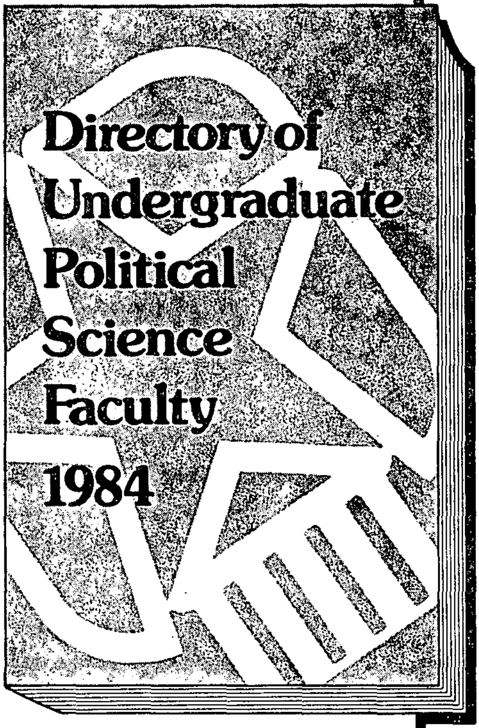

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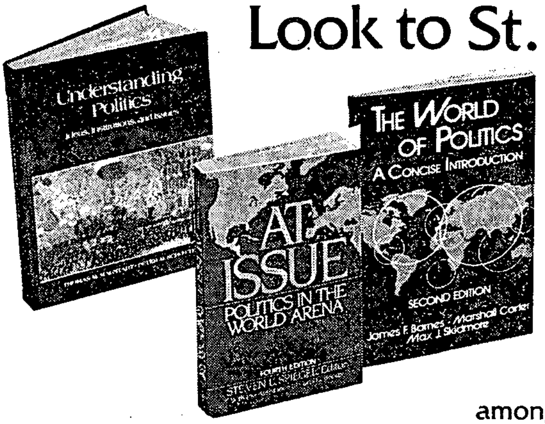

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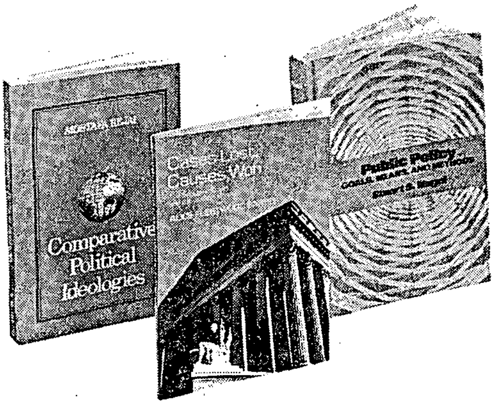

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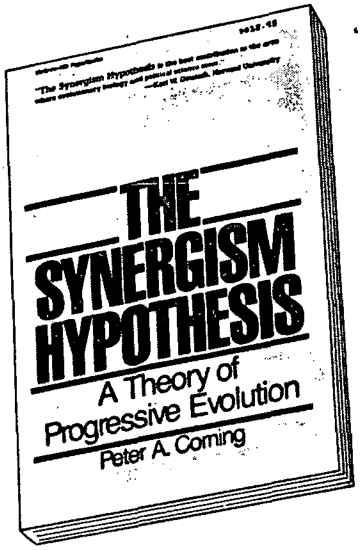

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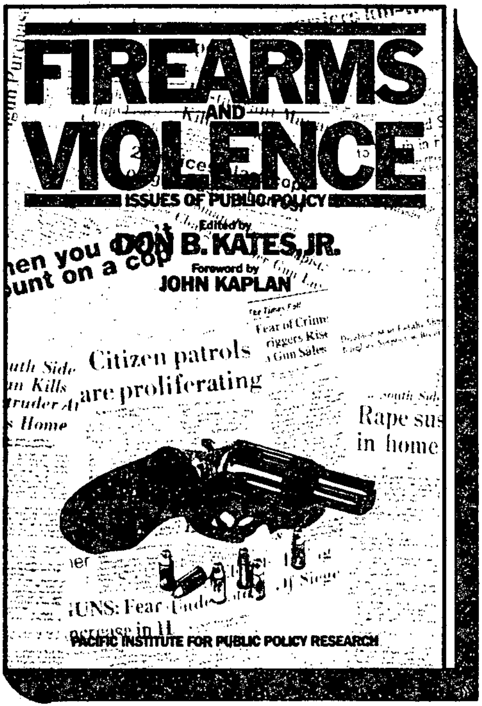
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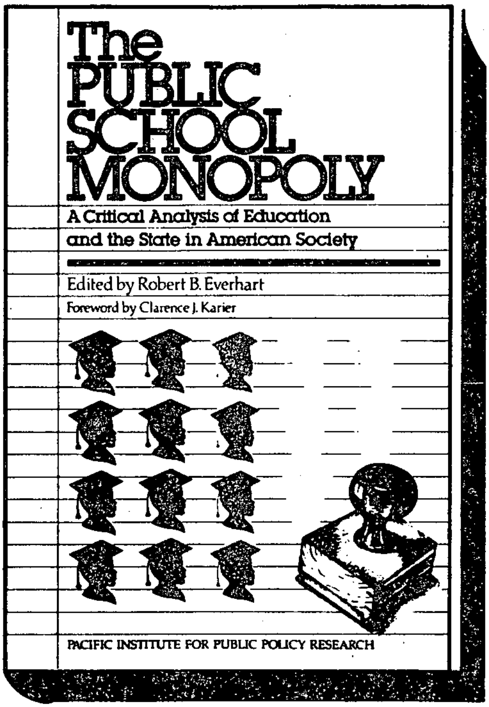
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